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PROCEEDINGS

OF

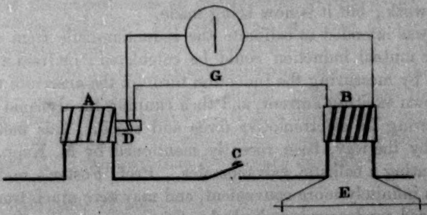
THE PHYSICAL SOCIETY

OF LONDON.

JULY 1887.

I. *Note* on Prof. Carey Foster's *Method of Measuring the Mutual Induction of two Coils. By* JAMES SWINBURNE[*].

PROF. FOSTER suggests at the end of his paper[†] that his method may be of use in dynamo work. Wishing to get some arrangement for testing the induction through any part of a dynamo, and for testing samples of iron for Messrs. Crompton and Co., the writer devised an arrangement which dispenses with the objectionable ballistic galvanometer. It was tried roughly last summer, with the view of making a permanent arrangement if the method worked well.



The primary current was led through an electromagnet, A, which represented the dynamo, and through one wire of a double-wound coil, B, which represented a pair of coils of known mutual induction. This coil was shunted by a wire, E, with sliding contacts. This circuit was made or broken by the switch C. A pilot wire, D, round the model electro-magnet was connected in series with the secondary coil of B and with the ordinary reflecting-galvanometer, G. The arrangement was fairly sensitive; but had one fault, which was in fact foreseen. Instead of no current, the galvanometer gets a current first one way and then the other, so that the spot jerks each way. This might be remedied by another arrangement of the wires; but a similar difficulty will arise in dynamo work. The flux of the induction through the standard coil varies as the flux of the primary current; but the flux of the induction through, say, a section of a field-magnet depends on the saturation of the iron. To avoid these difficulties, a galvanometer with a heavy ballistic astatic needle of the kind introduced by Ayrton and Perry will be used. This has been made by Mr. Dobson, lately one of Messrs. Crompton's pupils. It is of somewhat novel design. The coils, of which there are several sets, can be changed without dismounting the needle, so that the instrument can be used as a high or low resistance or differential galvanometer; and can be also used as a ballistic, or as a flywheel or integrating galvanometer. If necessary, little arms with balls like those of a minute fly-press will be added. The final apparatus was not put in hand till quite lately, as there was no suitable room available for such work; but it is now being made.

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[*] Read February 26, 1887.

[†] Proc. Physical Society, vol. viii. p. 137.

It was intended to calibrate the induction-coils from coils whose mutual induction could be calculated; or from a dynamo by measuring the induction through the armature with a known exciting current, and then running the dynamo and measuring its electromotive force and speed. This method has, by the way, been recently mentioned by M. Kapp, for calibrating a ballistic galvanometer. Prof. Foster's method seems infinitely more convenient, and may save much trouble if a good condenser is to be had. Of course an ammeter will be in the primary circuit.

It would seem that a flywheel-galvanometer might be used for such purposes as finding the ohm from a pair of coils with calculated mutual induction. Suppose, for instance, a contact-breaker makes and breaks contact in the primary circuit with a known frequency, and allows the impulses one way in the secondary coil to go round one coil of a differentially-wound flywheel-galvanometer. A constant current in the other coil is regulated to oppose these impulses; and from these the ohm might be got. Either the contacts must be made slowly enough for the current in the primary to come to its permanent value, or a correction must be made for the error. There is also a small error due to capacity. This suggestion is made with hesitation, as it seems probable that somebody must have thought of the arrangement or tried it. It is infinitely easier to think of a novelty, or an apparent novelty, than to find out whether it is really new. For instance, on looking the matter up a little for this note, the writer found that the null method of comparing the coefficients of mutual induction of coils is mentioned in a footnote to Brillouin's elaborate memoir on the comparison of coefficients of induction. Maxwell also mentions a loaded differential galvanometer for comparing frequent condenser-discharges with a known current. Roïti's method is also given in Mascart and Joubert's book.

II. *On the Electrical Resistance of Vertically-suspended Wires.*

By SHELFORD BIDWELL, *M.A.*, *F.R.S.*[*]

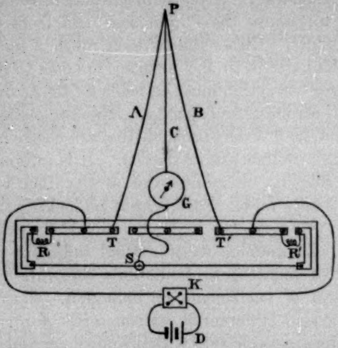
FROM the experiments to be described in this paper, it appears probable that the electrical resistance of vertically-suspended copper and iron wires alters to a small extent with the direction of the current traversing them. If the wire is of copper, the resistance is slightly greater when the current goes upwards than when it goes downwards; while, on the other hand, the resistance of an iron wire is apparently greater for downward than for upward currents[†].

---

[*] Read March 12, 1887.

[†] Venturing to imitate the fanciful analogy used by Sir William Thomson, who, in discussing the thermoelectric effect now universally associated with his name, speaks of the "specific heat" of electricity, we may perhaps also speak of the "specific gravity" of electricity, and say that (like its specific heat) it is positive in copper and negative in iron.

The arrangement employed for exhibiting this effect is shown in the annexed diagram. A wire, A B, of the material to be tested is suspended at its middle point, P, from a support 10·5 metres above a metre-bridge, to the terminals, T T′, of which the ends of the wire are connected. Another wire, C, is soldered at one end to P, and connected through the galvanometer, G, with the slider, S. A resistance of 100 ohms is inserted in each of the gaps, R R′, and a commutator, K, is interposed between the two-cell battery, D, and the bridge.



With this arrangement, supposing that the two halves of the wire A B are of uniform sectional area and in the same physical condition, and that the various parts of the apparatus are in fair order and adjustment, there will be a balance when the slider is near the middle division of the scale. And if the resistances in the circuit are independent of the direction of the current, it is clear that the balance will be maintained notwithstanding that the commutator K be reversed. But this is found not to be the case.

A series of experiments was made with a copper wire ·4 millim. in diameter (No. 28 B.W.G.), and having a total resistance of 2·11 ohms. The commutator was first set so that the current through the wire passed up the portion B and down the portion A (*i. e.* in the direction B P A), and a balance was obtained by adjusting the slider. The commutator was then reversed and the current made to pass up A and down B. This at once destroyed the balance, and in order to restore it, it was necessary to move the slider several divisions towards the right. Assuming that the total resistance of the wire remains constant, this result may be explained by supposing that the reversal of the current is accompanied by increased resistance in the portion A, and diminished resistance in the portion B. Owing to its vertical suspension, the resistance of that portion of the wire in which the current travels upwards is greater than it would be if the wire were placed in a horizontal position, while the resistance of the portion in which the current travels downwards is less.

The experiment was repeated with an iron wire of larger size, its diameter being ·8 millim. (No. 22 B.W.G.). With this the effect of reversal was smaller; but it was well marked, and of the opposite nature to that observed in the former case.

The readings obtained in the two series of experiments are given in the following Table :—

Copper Wire.			
Number of experiment.	Scale-readings.		Difference.
	Current direct.	Current reversed.	
1.	569	633	−64
2.	567	637	−70
3.	595	651	−56
		Mean difference	−63·3
Iron Wire.			
1.	780	770	+10
2.	760	748	+12
3.	759	748	+11
		Mean difference	+11

I believe these effects are associated with certain thermoelectric phenomena discovered by Sir William Thomson. In his famous Bakerian lecture, published in the Philosophical Transactions for 1856, he showed that if a stretched copper wire is connected with an unstretched wire of the same metal and the junction heated, a thermoelectric current will flow from the stretched to the unstretched wire through the hot junction; while, if the wires are of iron, the direction of the current will be from unstretched to stretched. It follows, therefore, from the laws of the Peltier effect, that if a battery-current is caused to flow from a stretched to an unstretched wire, heat will be absorbed at the junction when the metal is copper, and will be developed at the junction when the metal is iron: and if the direction of the current is reversed the thermal effects will also be reversed.

Now a vertically suspended wire is unequally stretched by its own weight, the stress gradually increasing from zero at the lowest point to a maximum at the highest. Any small element of the wire is more stretched than a similar element immediately below it, and less stretched than a neighbouring element just above it. Thus a current of electricity, in passing from the lowest to the highest point of such a wire, is always flowing from relatively unstretched to relatively stretched portions. If, then, the wire were of copper, heat would be evolved throughout its whole length; the temperature of the wire would rise, and its resistance would consequently be increased. With a current flowing from top to bottom, the temperature of the wire would fall and its resistance diminish. So also an iron wire would be cooled and have its resistance lowered by an upward current, while a downward current would heat it and increase its resistance. The changes of resistance are thus, as I believe, proximately due to changes of temperature.

The resistance of the bridge-wire used in my experiments was ·244 ohm, and, as already mentioned, an additional resistance of 100 ohms was placed in each of the gaps adjoining the bridge-wire. Denoting the resistance of the half A of the suspended wire by *a*, and that of B by *b*, we have, from the first experiment with the copper wire (the result of which agrees closely with the mean):—

For direct current,

$$\frac{a}{b}=\frac{100^{\omega}+{\cdot}569\times{\cdot}244^{\omega}}{100^{\omega}+{\cdot}431\times{\cdot}244^{\omega}}$$

$$=\frac{100139}{100105}.$$

Also

$$a+b=2{\cdot}11^{\omega}.$$

Hence

$a=1{\cdot}0551792^{\omega}$ *,

$b=1{\cdot}0548208^{\omega}$.

For reversed current,

$$\frac{a}{b}=\frac{100^{\omega}+{\cdot}633\times{\cdot}244^{\omega}}{100^{\omega}+{\cdot}367\times{\cdot}244^{\omega}}$$

$$=\frac{100154}{100090}.$$

And, as before,

$$a+b=2{\cdot}11^{\omega}.$$

Hence

$a=1{\cdot}0553372^{\omega}$*,

$b=1{\cdot}0546628^{\omega}$.

When therefore the current was reversed, the value of *a* was increased by

$$1{\cdot}0553372-1{\cdot}0551792 \text{ ohm}$$

$$={\cdot}000158 \text{ ohm}.$$

This is equivalent to about 16 thousandths per cent.

Assuming that a change of temperature of 1° C. produces an alteration of ·4 per cent. in the resistance, it follows that the temperature of the copper wire was $\frac{1}{25}$ degree C. higher with an upward than with a downward current.

The current traversing the wire was not measured, but it was probably about 1 ampere.

It will be seen from the figures in the Table that the changes which occurred in the resistance of the iron wire were considerably smaller than those observed in the case of copper. This was unexpected, since the thermoelectric effects are, I believe, somewhat greater with iron. But the apparent anomaly is obviously to be accounted for, at least in part, by the higher specific resistance of iron. With the same electromotive force the current per unit of sectional area would be six or seven times greater in copper than in iron, and the Peltier effect is proportional to the current. To render the results in the two cases strictly comparable, other less important differences, such as those of specific heat and radiating-power, would have to be taken into account.

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* Of course the resistances are not really measured to the high degree of accuracy suggested by these figures; but any small error of excess or defect would be approximately the same for the two values of *a* (with direct and reversed currents) and would not materially affect their difference, to which alone importance is attached.

If a convenient opportunity offered it would be satisfactory to repeat the experiments with much longer wires, such as might be suspended in the shaft of a coal-pit or in a shot-tower. The effects hitherto observed are so small that they might possibly be due to accidental causes, and I publish this account of them with some diffidence.

III. *On the Production, Properties, and some suggested Uses of the Finest Threads. By* C. V. BOYS, *Demonstrator of Physics at the Science Schools, South Kensington* *.

I HAVE lately required for a variety of reasons to have fibres of glass or other material far finer than ordinary spun glass; I have therefore been compelled to devise means for producing with certainty the finest possible threads. As these methods may have some interest, and as some results already obtained are certainly of great importance, I have thought it desirable to bring this subject under the notice of the Physical Society, even though at the present time any account must of necessity be very incomplete.

The subject may be naturally divided, as in the title, into three parts.

1. *Production.*

The results of the natural methods of producing fibres by living things, as spiders, caterpillars, and some other creatures, are well known ; but it is useless to attempt to improve on Nature in this direction by our own methods.

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* Read March 26, 1887.

Fibres are also produced naturally in volcanoes by the rushing of steam or compressed gases past melted lava, which is carried off and drawn out into the well-known Pelés hair. The same process is employed in making wool from slag, for clothing boilers, &c. ; but in each of these cases the fibres are matted together, they are not adapted to the requirements of a Physical Laboratory. By drawing out glass softened by heat by a wheel we obtain the well-known spun glass.

There is a process by which threads may be made which is natural in that natural forces only are employed, and the thread is not in any way touched during its production. This is the old, but now apparently little-known experiment of electrical spinning. If a small dish be insulated and connected with an electrical machine and filled with melted rosin, beeswax, pitch, shellac, sealing-wax, Canada balsam, guttapercha, burnt india-rubber, collodion, or any other viscous material, the contents will, if they reach one edge of the dish, at once be shot out in the most extraordinary way in one, two, or it may be a dozen threads of extreme tenuity, travelling at a high speed along "lines of force." If the material is very hot, the liquid cylinders shot out are unstable and break into beads, which rattle like hail on a sheet of paper a few feet off. As the material cools, the beads each begin to carry long slender tails, and at last these tails unite the beads in twos and threes ; but the distance between the beads is far greater than that due to the natural breaking of a cylinder into spheres, as after the first deformation of the surface occurs which determines the ultimate spheres the repulsive force along the thread continues, and drags them apart many times their natural distance. As the temperature continues to fall and the material to become more viscid, the beads become less spherical, and the tails less slender, and at last a perfectly uniform cylindrical thread is formed. If sealing-wax is employed, and a sheet of paper laid for it to fall on, the paper becomes suffused in time with a delicate rosy shade produced by innumerable fibres separately almost invisible. On placing the fingers on the paper, the web adheres and can be raised in a sheet as delicate and intricate as any spider's-web.

It is interesting to see how these fibres fly to any conducting body placed in their path. If the hand is held there it is quickly surrounded by a halo of the finest threads. If a lighted candle is placed in the way, the fibres are seen by the light of the candle to be rushing with the greatest velocity towards it, but when a few inches off they are discharged by the flame, they stop, turn round, and rush back as fast into the saucer whence they came. The conditions for the success of this beautiful experiment are not very easily obtained*.

Fibres spun by the electrical method are so brittle that they do not seem to be of any practical use. It is possible, however, that this method might be available for reducing to a fine state of division such of the rosins or other easily fusible bodies as cannot readily be powdered mechanically.

On returning to bodies which, like glass, require a high temperature for their fusion, to which the electrical method is inapplicable, we find that the only method practically available is that of drawing mechanically. It would seem that if finer threads than can be formed by the ordinary process of glass-spinning were required, it would be necessary to obtain a higher speed, to have the glass hotter, and to have as small a quantity as possible hot. I put this idea to a test by mounting at the back of a blowpipe-table a pair of sticks which could be suddenly moved apart by a violent pull applied to each near their axes. By these means the upper ends were separated about 6 feet, and the motion was so rapid that it was impossible to follow it. A piece of glass drawn out fine was fastened to the end of each stick, and the ends of these heated by a minute blowpipe-flame. They were immediately made to touch and allowed to fly apart. In this way I obtained threads of glass about 6 feet long, finer than any spun glass I have examined. By using the oxyhydrogen jet with the same apparatus, still finer threads were produced. It was evident then that the method was right; but some more convenient device which also would make long threads would be preferable.

There are several ways of obtaining a high speed, the most usual depending on an explosive; but it would be difficult to arrange in a short time a gun which could be used to shoot a projectile carrying the thread which would not also destroy the thread by the flash. It is possible that an air-gun could be so arranged. Rockets when at the period of most rapid combustion have an acceleration which is enormous. Thus a well-made 2-oz. rocket is at one part of its flight subject to a force of over 3 lb. in gravitative measure. This force, acting on such a body for 10 seconds only, would, neglecting atmospheric resistance, starting from rest, carry it more than 6 miles. The acceleration is about 28 times that due to gravity on the earth, or about the same as that on the sun. Anyone who will stay in a room with a lighted two-ounce rocket, having no stick or head, will obtain a more vivid notion of the value of gravity on the sun than in any other way I know.

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* If the wick of the candle is connected with the opposite pole of the machine, the threads at one stage are sure to return to the saucer.

A rocket is perhaps more available for thread-drawing than a gun, but it does not seem altogether convenient. One other method, however, is so good in every respect, that there seems no occasion to try a better. The bow and arrow at once supply a ready means of instantly producing a very high velocity, which the arrow maintains over a considerable distance. For the special purpose under consideration, the lightest possible arrow is heavy enough. I have made arrows of pieces of straw, which may be obtained from wool-shops, a few inches long, having a needle fastened to one end for a point. Arrows made in this way travelled the length of the two rooms in which I made these experiments—about 90 feet—in what seemed to be under half a second. They completely pierced a sheet of card at that distance, which I put up thinking that a yielding target might damage them less than the wall, and were then firmly stuck unharmed in the wall behind; in every way they behaved so well that I do not think a better make of arrow possible.

The bow I used was a small cross-bow held in a vice with a trigger that could be pulled with the foot. The first bow was made of oak, the first wood that came to hand. I then made some bows of what was called lance-wood (it was unlike any lance-wood I have seen); but the trajectory was at once more curved, the arrow took perceptibly longer to travel, and the threads produced were thicker. As the arrow is so light, the only work practically that the bow has to do is to move itself; that wood then which has the highest elasticity along the fibres for its mass is most suitable; in other words, that wood which has the greatest velocity of sound is best. I therefore made bows of pine, and obtained still higher velocities and finer threads than I could obtain with oak bows.

With a pine bow and an arrow of straw I have obtained a glass thread 90 feet long and $\frac{1}{10000}$ inch in diameter, so uniform that the diameter at one end was only one sixth more than that at the other. Pieces yards long seemed perfectly uniform.

A fragment of drawn-out glass was attached to the tail of the arrow by sealing-wax, and heated to the highest possible temperature in the middle, the end being held in the fingers. With every successful shot the thread was continuous from the piece held in the hand to the arrow 90 feet off. The manipulation is, however, difficult, but another plan equally successful has the advantage of being quite easy. It is not necessary to hold the tail of glass at all; if the end of the tail only be heated with the oxyhydrogen jet until a bead about the size of a pin's head is formed, and the arrow shot, this bead will remain behind on account of its inertia, and the arrow go on, and between them will be pulled out the thread of glass.

Prof. Judd has kindly given me a variety of minerals which I have treated in this manner. Some behave like glass and draw readily into threads, some will not draw until below a certain temperature, and others will not draw at all, being either perfectly fluid like water, or when a little cooler perfectly hard.

Among those that will not draw at all may be mentioned Sapphire, Ruby, Hornblende, Zircon, Rutile, Kyanite, and Fluorspar.

Emerald and Almandine will draw, but care is required to obtain the proper temperature. In the case of the Garnet Almandine, if the temperature is too high, the liquid cylinder, if formed, breaks up, and a series of spheres fall on the table in front of the bow. At a slightly lower temperature the thread is formed, but it is beaded at nearly regular intervals for part of its length.

Several minerals, especially complex silicates as Orthoclase, draw very readily, but that which surpasses all that I have tried at present is Quartz, which, though troublesome in many ways at first, produces threads with certainty. It required far more force to draw quartz threads than had been previously experienced. The arrow, instead of continuing its flight, hardly disturbed by the drag of the thread, invariably fell very low, and was not in general able to travel the whole distance. So great is the force required that I split many arrows before I succeeded at all. I have obtained threads of quartz which are so fine that I believe them to be beyond the power of any possible microscope. Mr. Howes has lent me a $\frac{1}{15}$-in. Zeiss of excellent definition, and though, on looking at suitable objects, definite images appear to be formed on which are marks corresponding according to the eyepiece-micrometer to $\frac{1}{100000}$ inch, yet these threads are hopelessly beyond the power of the instrument to define at all. On taking one that tapers rapidly from a size which is easily visible, the image may be traced until it occupies a small fraction of one division, of which 13·4 correspond to $\frac{1}{1000}$ inch on the stage; then the diffraction-bands begin to overlap the image until it is impossible to say what is the edge of the image. Having reached this stage, the thread may be traced on and on round the most marvellous convolutions, the diffraction-fringe now alone appearing at all, but getting fainter and apparently narrower until the end is reached. That a real thing is being looked at is evident, for if the visible end is drawn away the convolutions of fringes travel away in the same direction. It is impossible to say what is the diameter of these threads; they seem to be certainly less than $\frac{1}{100000}$ inch for some distance from the end.

It might be possible to calculate what would be the appearance presented by a cylinder of given refractive power, and 1, 2, 3, &c. tenths of a wave-length of any kind of light in diameter, when seen with a particular microscope. By no other means does it seem possible to find out what the true size of the ends of these threads really is.

2. *Properties.*

I can at present say very little of the properties of these very fine fibres; I am now engaged with Mr. Gregory and Mr. Gilbert in investigating their elasticity. The strength goes on increasing as they become finer, that is, when due allowance is made for their reduced sectional area, and it seems to reach that of steel, about 50 tons to the inch in ordinary language; but on this point I have not yet made any careful experiments.

The most obvious property of these fibres is the production of all the colours of the spider-line when seen in a brilliant light. The most magnificent effect of this sort I have seen, was produced by a thread of almandine. One of these the length of the room, even though illuminated with gas-light only, was glistening with every colour of the rainbow. In attempting, however, to wind it up, it vanished before me. It is of course only visible in certain directions.

The chief value of threads to the physicist lies in their torsion. Spun glass, as is now well known, cannot be used for instruments of precision, because its elastic fatigue is so great that, after deflection, it does not come back to the original position of rest, but acquires a new position which perpetually changes with every deflection. If left alone, this position slowly works back towards a definite place more rapidly as it is further from it.

To compare threads made of different materials, I made a flat cell in which a galvanometer-mirror, made by Elliot Bros., might hang, being attached to the lower end of the thread. The upper end was secured to a fixed support, and a fixed tube protected the length of the fibre from draught. The cell, which could be moved independently of the rest, was protected by a cover. By means of a lamp and scale, the exact position of rest of the mirror could be determined with great accuracy. On turning the cell round as many times as might be desired, the mirror was turned with it, and could be left any time in any position. On turning the cell back again, the mirror was allowed to come to its new position of rest, air-resistance of the cell bringing about this result in a few swings. By this means I hoped quickly and accurately to determine the fatigue in a variety of threads, but an unforeseen difficulty arose which I cannot yet explain. When the cell was moved round slightly so as not to touch the mirror, the mirror moved at first in the same direction as was to be expected, but it came to rest in a new position, to reach which it had to move in the opposite direction to the movement of the cell. Whichever way the cell was shifted, the mirror always went the other to find its position of rest. Thinking that it or the cell were electrified, I damped both by breathing on them, but with no result, and the next day the same effect was observable. So great was this effect that I could set the cell with greater accuracy by watching the spot of light than by the pointer carried by the cell working over a 4-inch circle.

Thinking that magnetism might have something to do with this effect, I brought a horseshoe-magnet near the mirror, when it was instantly deflected through a large angle. An examination of the cement used (Loudon's bicycle cement) showed that it was magnetic. Of many cements examined, sealing-wax was more nearly neutral than any other. Bicycle cement and electrical cement were strongly magnetic; all others except sealing-wax strongly diamagnetic. The apparatus was therefore taken to pieces and carefully cleaned. It was put together with as small a quantity of sealing-wax as possible, and the mirror was attached to a fragment of thin pure copper wire, which again was fastened by a speck of sealing-wax to the thread. Even then the same kind of effect as that already described occurred. Still a magnet deflected the mirror, but not so much, and the cell was practically neutral; yet, when the cell was turned a little, the mirror changed its position of rest.

Without pursuing this question further, I put a window in the protecting tube and turned the mirror by means of a small instrument passed up from below. Thus neither window nor support were moved. A piece of spun glass nearly 9 inches long gave a period of oscillation to the mirror of 2·3 seconds about. A lamp and millimetre-scale were placed 50 inches from the mirror. As all the observations were expressed in tenths of a millim., to about which extent they can be trusted, it is convenient to employ one scale of numbers of which one tenth millim. is the unit. One complete turn of the mirror is very nearly 160,000 on this scale. If the mirror is moved through 160,000 in either direction and held for one minute, and then allowed to take its new position, the change in the position of rest is as soon as it can be read about 370. This is reduced in about three minutes to 110. If the mirror is moved through three turns, 480,000 of the scale, and held one minute, the position of rest is at first moved about 1100, which falls in three minutes to about 400. I have given these figures, not because the effect is not perfectly well known, but to serve as comparison figures to those that are to follow. They can only be properly represented on a time-diagram.

A piece of the same fibre that was used in the last experiment was laid in a box of charcoal and heated in a furnace to a dull red heat and allowed to cool slowly. This was examined in the same way as the last. The effect of a movement of 160,000 for one minute was now only about 60, which was reduced to about 45 in three minutes. The change for 480,000 lasting one minute was at first about 250, which fell to about 180 in three minutes.

Annealed spun glass then shows far less of this effect than spun glass not annealed, but it is slower in recovering. It is possible that if time were given, it would show as great an effect as plain glass. The only mineral from which at the present time I have obtained any valuable results in this direction, is quartz. Here the effect of the usual minute at 160,000 was only 7, in the place of 370 for glass, at 320,000 only 17, and 640,000 only 32, which in four minutes fell to 22. This fibre was as usual fastened at each end by sealing-wax. When this experiment was made, the thread had only just been fastened. The same fibre treated previously in the same way, but some days after fastening, did not even show this effect; but as this was before I had completed the proper cell, the observations cannot so well be trusted. After a complete turn, there was not a movement of one tenth of a millim., nor had the position changed this much in 16 hours. It is as yet too soon to be sure, but this seems to point to the possibility of the very slight effect observed being largely due to the sealing-wax. Whether this is so or not does not much matter, the behaviour of the quartz thread approaches sufficiently near to that of an ideal thread, to make it of the utmost value as a torsion-thread. I hope shortly to be able to bring results of carefully conducted experiments on the elastic fatigue of quartz and other fibres before the notice of this Society.

A thread of annealed quartz behaves like a thread of quartz not annealed. That it was affected by the process of annealing is evident, because in the first place it was very rotten and difficult to handle, and in the second a piece of quartz fibre, which was wound up, retained its form. By this test, quartz can only be partly annealed in a copper box, as any form is not retained perfectly; at a temperature above that of melting copper, quartz seems to perfectly retain any form given to it.

It is probable that a body hung by a fibre of quartz and vibrating in a perfect vacuum would remain twisting backwards and forwards for a far longer time than a similar body hung by a glass thread, also that the most perfect balance-spring for a watch would be one of quartz. I have a piece of quartz drawn out to a narrow neck which just cannot hold up its head; this keeps on nodding in all directions for so long a time, even in the air, as to make it evident that the material has very unusual properties.

3. *Uses.*

As torsion-threads these fibres of quartz would seem to be more perfect in their elasticity than any known; they are as strong as steel, and can be made of any reasonable length perfectly uniform in diameter, and, as already explained, exceedingly fine. The tail-ends of those that become invisible must have a moment of torsion 100 million times less than ordinary spun glass; and though it is impossible to manipulate with those, there is no difficulty with threads less than $\frac{1}{10000}$ inch in diameter.

I have made a spiral spring of glass of about 30 turns which weighs about one milligram; this, examined by a microscope, would show a change in weight of a thing hung by it of one 10 millionth of a gram. Since this has been annealed its elastic fatigue is that of annealed glass, and therefore very small. I have succeeded in doing the same thing with a quartz fibre, but the difficulties of manipulation are very great in consequence of the rottenness of annealed quartz. The glass spring can be pulled out straight, and returns perfectly to its proper form.

Since these fibres can be made finer than any cobweb, it is possible that they may be preferable to spider-lines in eye-pieces of instruments; they would in any case be permanent, and not droop in certain kinds of weather.

Those who have experienced the trouble which the shifting zero of a thermometer gives, might hope for a thermometer made of quartz. When made, it would probably be more perfect in this respect than a glass thermometer, but the operation of making would be difficult.

These very fine fibres are convenient for supporting small things of which the specific gravity is required, for they weigh nothing, and the line of contact with the surface of the water is so small, that they interfere but little with the proper swing of the balance.

It seemed possible that a diffraction-grating made of fibres side by side in contact with one another would produce spectra which would be brighter than those given by a corresponding grating of ordinary construction, because not only is all the light which falls on the surface brought to a series of linear foci forming the bright lines instead of being half removed, as is usually the case, but the direction of the light on reaching these lines is not normal to the grating as usual, and therefore in the direction of the central image, but spreading, and thus in the direction of all the spectra. I picked out a quantity of glass fibre not varying in diameter more than one per cent., and made a grating in this way covering about one eighth of an inch in breadth. This not only showed three spectra on each side, and a quantity of scattered light, but all the spectra were closely intersected by interference-bands, such as are seen when a Newton's ring of a high order is seen in a spectroscope. This is probably due to a cumulative error in the position of the fibres, for they were spaced by being pushed up to one another with a needle-point, or to light passing between the fibres in a few places where dust particles keep them apart.

A diffraction-grating made of these fibres, spaced with a screw to secure uniformity, and of a thickness equal to the spaces between them (and one of 1000 lines to the inch could be easily made) would be far more perfect for the number of lines than any scratched on a surface; that is, for investigation on the heat of a spectrum, such a grating would be preferable to a scratched one, as there is no uncertainty as to the grating or to the substance of which it is made [*]. If the transparency of the fibres interfered they could be rendered opaque by metallic deposit without visibly increasing their diameter.

There is one use to which the fibres of quartz tailing-off to a mere nothing might be applied, namely as a test-object for a microscope. Theory shows that no microscope can truly show any structure much less than $\frac{1}{150000}$ inch, or divide two lines much less than this distance apart. Natural bodies such as Diatoms &c. have this advantage, that they can be obtained in any quantity alike, but no one knows what the real structure of these may be. Nobert's bands are good in that we know the number of lines in any band, but as to the individual appearance of the lines and spaces it is impossible to say anything. These fibres have the advantage that we have a single thing of known form, which tapers down from a definite size to something too small even to be seen. Though it may be possible to calculate the size from the appearance of the fringes, yet whether the size is known or not, at each point we have a definite thing of known form which can be examined by a series of microscopes, and the point up to which it can be clearly seen observed for each.

I have thought it worth while to bring this subject forward in this very incomplete form, because there are already results of interest and there is so much prospect of more, that it is likely that Members may be glad to investigate some of the questions raised.

IV. *Note on Magnetization.—On Sequences of Reversals.*

By R. H. M. Bosanquet [†].

The present paper arose out of a recent discussion, at a meeting of the Physical Society, on Prof. Ayrton's paper on the magnetic resistance of a broken ring. I alluded on that occasion to the determinations of the magnetism of rings and bars which I had made. In answer to a question as to the course I had pursued as to ascending or descending values of magnetism, I stated that in all cases I had taken the values in the ascending order. But I omitted to make it clear that the observations were made by reversal, so that the direct effects of residual magnetism were necessarily eliminated. The arrangement I have always employed may be described as an ascending sequence of reversals.

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[*] See 'Heat,' by Prof. Tait, p. 268.

[†] Read April 23, 1887.

There can be no doubt, however, that, in sequences of reversals, the history of previous magnetizations produces some effect. I still consider that the best way of attacking the problem is to take the first ascending sequence, in which the history is a minimum, and then as a subsequent study to deal with the effects of the history.

Immediately after the discussion above alluded to I made a few experiments on the first bar I ever completely determined, with the view of ascertaining the general character presented by series of successive reversals. The original determinations will be found at Phil. Mag. (1884) xvii. pp. 531–6, Soft-Iron Bar I.

I shall confine myself at present to the values of magnetic resistance. The reason for preferring this datum is that, when plotted with the induction as abscissa, the form of the curve of magnetic resistance is approximately independent of the arrangement of the metal, the only material differences occurring in the region of saturation. Thus values for bars and rings differ only by a constant, which may be regarded as due to the shape. Bars with pole-pieces have similar curves lying between those for bars and rings. See Phil. Mag. xxii. p. 303, and the paper above referred to.

I shall call the old experiments the first ascending sequence (A). Then I made a series which I shall call the second ascending sequence (B). Then a final descending sequence (C).

The first values of B, which follow the old saturation, have the resistance somewhat higher than that found when the bar was fresh. After a few reversals, however, the effect of the history seems to disappear as the values ascend, and the value for mean inductions (minimum value of resistance) is almost exactly the same as before. The saturation-value corresponds to a higher induction than would be given by the old curve.

The succeeding curve of descending values (C) shows increased resistance, or less magnetism, in the mean inductions.

As the small inductions are approached the curve of C crosses the old curve, and ends with a lower resistance, or greater magnetism than the original initial value.

The effects of ascending and descending sequences of reversals on the initial values would therefore appear to be opposite in direction. At the same time the recent experiments are few in number, and a much more extensive course of work will have to be done before conclusions on these points can be drawn with generality.

I should like to say a few words on the relation of results of this description to the molecular hypothesis, by means of which I have represented a large number of experiments. First, as to the position in which the hypothesis stands.

I am quite unable to understand how Weber's hypothesis can be applied to account for such laws as we are dealing with, *i. e.* where the initial magnetic resistance is greater than that for mean inductions. The hypothesis, which I have worked out and applied in detail to a large number of experimental cases of all sorts*, depends on two chief processes; the one of which accounts chiefly for the larger initial values of the magnetic resistance, the other for the larger saturation-values.

First, as to the saturation-values. Each magnetic particle is supposed to transmit the magnetism through a certain axis and not otherwise. Using the analogy of a hole in a bead, packed with wires, the permeability so far is measured by the portion of the hole left unoccupied, or by what I call the defect of saturation. This alone would lead to a law of permeability similar to Frölich's supposed law of the conductivity of magnets with ends.

Next, to account for the initial values. The axes of the particles being distributed uniformly in all directions, there will be an action arising from the tension of the lines of force tending to draw all the axes towards the mean direction of the magnetism; and thus the length of the path, and consequently the resistance, will be diminished as the magnetism increases. This has all been worked out in detail and shown to be capable of representing the facts minutely, in the papers referred to. All I want to show here is that we can refer our results as to increase or diminution of resistance by sequences of reversals to this theory. For the permeability would be diminished by partially filling up the saturation-capacity, and increased by partially fixing the molecules in positions of diminished resistance. All magnetization tends to retain the latter change in the same direction; *i. e.* to diminish the mean inclination of the axes, and consequently the resistance. But successive reversals of diminishing currents are capable of removing the greater part of the magnetism that sticks in the particles (subpermanent magnetism). This accounts generally for the observation that immediately after saturation the resistance is increased, but when a descending sequence of reversals is interposed the resistance is diminished. For the large residual charge after saturation diminishes the permeability to an extent exceeding the effect of inclination of axes. But when the residual charge is dispersed by the reversals of a descending sequence, the effect of the diminished inclination of the axes remains, and increases the initial permeability.

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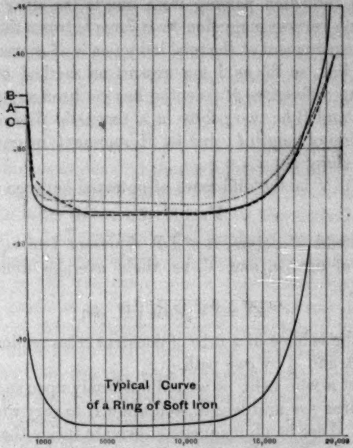
* Phil. Mag. (1885) xix. pp. 73 & 333; xx. p. 318; xxii. p. 298; xxiii. p. 350.

The figure and Tables which follow exhibit the results of the experiments in question.

Soft-Iron Bar : ·938 centim. radius,
28·55 centim. length.

A. Original ascending sequence.		B. Second ascending sequence.		C. Descending sequence.	
$\mathfrak{B}$.	ρ.	$\mathfrak{B}$.	ρ.	$\mathfrak{B}$.	ρ.
	centim.		centim.		centim.
25	·343	14	·356	15	·328
168	·315	106	·342	237	·289
414	·272	258	·314	978	·254
1,423	·237	276	·305	2,529	·250
2,467	·238	384	·286	5,577	·246
10,210	·233	1,244	·257		
17.214	·299	4,384	·231		
17,858	·331	19,396	·385		
19,289	·445				
19,650	·552				

Sequences of Reversals of Magnetism of Soft-iron Bar.
ρ = magnetic resistance, in centimetres.



Bar A ——— original ascending sequence.
Bar B - - - - - second ascending sequence.
Bar C . . . . . . descending sequence.

The length of the ring is supposed to be the same as that of the bar; *i. e.* 28·55 centim.

V. *On a Magnetic Potentiometer.* *By* A. P. CHATTOCK[*].

IN arranging some experiments on the magnetic resistances (so-called) of certain air and iron fields with a view to the more satisfactory designing of dynamos, I have been led, by the familiar analogy between magnetic and electrical circuits, to adopt the following method of measurement; which I venture to describe, partly on account of its convenience, partly because the measurement of magnetic resistances seems likely to play an important part in the practical application of electromagnetism.

[*] Read May 14, 1887.

The resistance between two points on a magnetic circuit may be expressed as the ratio of their potential difference to the total induction passing from one to the other (provided there is no reverse magnetomotive force between them).

The measurement of the total induction is of course a simple matter; but, so far as I am aware, no method of directly measuring differences of potential has yet been suggested.

Let A and B be two points in a magnetic field connected by any line of length l; and let H represent magnetic force resolved along l.

Then, if V be the difference of potential between A and B,

$$V=\int H\,.\,dl.$$

If, instead of points, A and B represent two equal plane surfaces of area a, and $\overline{V}$ be their average difference of potential,

$$a\overline{V}=\int V\,.\,da=\int H.\,dv,$$

v being the volume of a tube of constant cross section, a, connecting A and B by *any* path.

Now let a wire helix be wound uniformly upon such a tube, with n turns per unit length, and allow H to vary with time, t. Provided there be no magnetic substance inside the helix, an electromotive force, E, will be set up in the latter, such that

$$E=\frac{d}{dt}\iint H\,.\,da\,.\,dn=\frac{d}{dt}\,n\int H\,.\,dv=na\frac{d\overline{V}}{dt}.$$

The value of E is thus proportional to the rate of change of $\overline{V}$; and to this *alone*, if external inductive effects are guarded against by winding the wire in an even number of layers (n and a being constant). Hence, if the wire be connected with a ballistic galvanometer, and $\overline{V}$ be altered suddenly from V_1 to V_2, the needle of the galvanometer will be thrown through the angle θ such that

$$V_1-V_2=K\sin\frac{\theta}{2},$$

and the combination forms what may be called a magnetic potentiometer.

In exploring a permanent field with such an apparatus, the best way is, perhaps, to fix one end of the helix in a clip, thereby keeping its potential constant, and to move the other end from point to point in the field. For this purpose the wire should be wound upon a flexible core, the average length of which, whether bent or straight, must be constant (otherwise $da \,.\, dn$ will not be equal to $n \,.\, dv$ in the last equation).

I have therefore constructed a helix by winding wire uniformly on to a piece of solid indiarubber, of constant cross section (in my case 37 centim. long and about 1 centim. diameter), leaving a small space between one turn and the next to allow the indiarubber to bend without elongating.

With this apparatus I made the following measurements of the potential difference between the ends of a permanent bar-magnet, in order to test the accuracy of the method. In the first set of readings the free end of the helix was moved at one leap from end to end of the magnet, giving a mean reading of 6·017; in the two other sets it was moved between the same two points in two and four leaps respectively, the resulting readings being added together in each case. The means of these were 6·047 and 6·048. The final mean was thus 6·037; and from this the most discordant reading differed by about 1 per cent. The close agreement between the second and third values was, no doubt, accidental; but the difference between them and the first may have been due to the difficulty of moving quickly from end to end of the magnet, a distance of over 40 centim.

In order to reduce these results to absolute measure, the helix (still connected with the galvanometer) was subjected to a known magnetomotive force by passing it through a coil of n turns and bringing its ends close together outside the coil, a current C being then started or stopped in the latter. The magnetomotive force due to this was of course equal to $4\pi n\mathrm{C}$, and this being substituted for $\mathrm{V}_1 - \mathrm{V}_2$ in the last equation determined the value of K. In my case these values were $n = 20$, $\mathrm{C} = 0{\cdot}182$ C.G.S., and galvanometer throw $= 0{\cdot}34$. The difference of potential between the ends of the magnet was thus

$$\frac{4\pi \times 20 \times 0{\cdot}182}{0{\cdot}34} \times 6{\cdot}037 = 810 \text{ C.G.S.}$$

A more suitable core for the helix than indiarubber is the flexible gas-tubing made of plaited and varnished canvas. It is very uniform in cross section; and by withdrawing the metal spiral upon which it is woven and mounting it on a spindle in a screw-cutting lathe, it is easy to wind the wire uniformly upon it.

The use of the lathe is the more desirable, as measurements of potential by the helix depend very much for their accuracy upon the uniformity with which it is wound; this being especially the case if its position in the field does not happen to coincide with the direction of the lines of force.

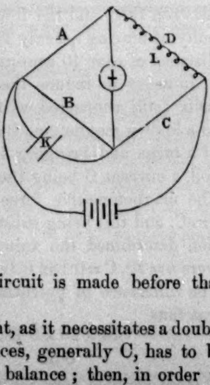
To keep the turns in place the small spaces between them may be filled with soft cotton-thread.

University College, Bristol.

VI. *On a Modification of a Method of* Maxwell's *for Measuring the Coefficient of Self-induction. By* E. C. Rimington[*].

In 'Electricity and Magnetism,' § 778, vol. ii., Maxwell gives a method of comparing the coefficient of self-induction of a coil with the capacity of a condenser, the connexions for which are practically as follows:—D is the resistance, possessing self-induction; the armatures of a condenser, of capacity K, are connected to the ends of the arm B. There are keys in the battery and galvanometer-circuits. An ordinary balance is obtained by depressing the battery-key before the galvanometer-key; then AC=BD. The resistances B and D are next adjusted so that no momentary deflection shall exist on the galvanometer when its circuit is made before that of the battery. Then L=KBD.

Fig. 1.



This method is very inconvenient, as it necessitates a double adjustment. One of the resistances, generally C, has to be adjusted so as to give an ordinary balance; then, in order to obtain no deflection when the galvanometer-circuit is closed, first the resistance B will have to be altered, as D is generally the resistance of the coil. This necessitates a fresh adjustment of C, and so on.

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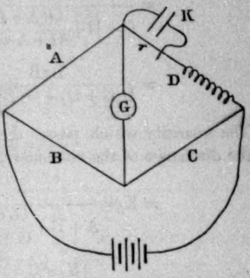
[*] Read May 14, 1887.

The first modification of this method I made was by putting a resistance possessing no self-induction in the arm D in series with the coil, as in fig. 2, a slider moving over this resistance and the condenser being connected as shown.

Fig. 2.

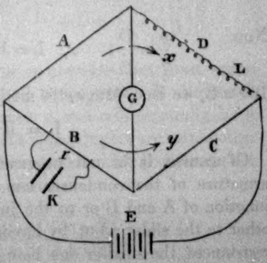


An ordinary bridge-balance is first obtained, and the slider is then adjusted until there is no throw on the galvanometer, when its circuit is closed before that of the battery. Then

$$L = Kr^2,$$

where r is the resistance between the armatures of the condenser. This method was found to be insensitive, as, unless K were large, r had to be made high. It will not do to make K too large with an ordinary mirror-galvanometer, on account of a sort of double throw being obtained, the effect of self-induction being more rapid than that of the condenser, and the galvanometer not ballistic enough. I then adopted the following modification, which answers very well.

Fig. 3.



The arm B is a resistance on which two sliders move, one of them being connected to each armature of the condenser K.

Obtain a permanent balance ; then AC=BD.

Now adjust sliders until there is also a balance when galvo.-circuit is closed first. Let x be the current flowing in the arms A and D when it has attained its permanent value, and let y be that in the arms B and C. Let r be the resistance between the sliders when both balances are obtained. Let the battery and galvanometer circuits be both closed, and let the former be broken. The quantity of electricity which passes through the galvanometer due to the self-induction in D

$$= \frac{Lx}{C+D+\frac{G(A+B)}{G+A+B}} \times \frac{A+B}{G+A+B}$$

$$= \frac{LxB}{B(C+D)+G(B+C)}, \text{ since } AC=BD.$$

The quantity which passes through the galvanometer, due to the discharge of the condenser,

$$= Kyr\frac{r}{A+B+\frac{G(C+D)}{G+C+D}} \times \frac{C+D}{G+C+D}$$

$$= \frac{Kyr^2C}{B(C+D)+G(B+C)}, \text{ since } AC=BD.$$

Now these quantities obviously pass through the galvanometer in opposite directions; and if there is no throw, they must be equal. Therefore

$$\frac{LxB}{B(C+D)+G(B+C)} = \frac{Kyr^2C}{B(C+D)+G(B+C)},$$

or

$$L = Kr^2\frac{y}{x}\cdot\frac{C}{B}.$$

Now $\frac{y}{x} = \frac{D}{C}$; $\therefore$ $L = Kr^2\frac{D}{B}$.

If $r = B$, we have Maxwell's method, and

$$L = KBD.$$

Of course it is not necessary to use two sliders; one armature of the condenser can be connected either to the junction of A and B or to the junction of B and C, and the other to the slider; but by having B composed of two slide-resistances, the smaller one being equal to the resistance of one of the coils of the larger, and employing a slider on each, a much greater range of adjustment can be obtained.

Suppose r to be slightly out of adjustment by an amount δ. Then the quantity which passes through the galvanometer, or

$$q=\frac{K(r+\delta)^2Cy-BLx}{B(C+D)+G(B+C)}.$$

Now $y=x\frac{D}{C}$;

$$\therefore q=x\frac{K(r+\delta)^2D-BL}{B(C+D)+G(B+C)},$$

and since $BL=Kr^2D$,

$$q=x\frac{2Kr\delta D}{B(C+D)+G(B+C)}.$$

If p be the fractional error in r, $\delta=pr$;

$$\therefore\ q=x\frac{2Kr^2Dp}{B(C+D)+G(B+C)}.$$

Now
$$x=\frac{E}{\rho+\frac{(A+D)(B+C)}{A+B+C+D}}\times\frac{C}{C+D}$$

$$=E\frac{C}{\rho(C+D)+D(B+C)},$$

since $AC=BD$; and $Kr^2D=BL$;

$$\therefore\ q=\frac{2ELpBC}{\{\rho(C+D)+D(B+C)\}\,\{B(C+D)+G(B+C)\}}$$

$$q=\frac{2Elp}{\left\{\rho\left(1+\frac{D}{C}\right)+D\left(1+\frac{B}{C}\right)\right\}\left\{C+D+G\left(1+\frac{C}{B}\right)\right\}}.$$

If we wish to make q as large as possible for a given value of p, we must make C small and consequently B also small. Of course B cannot be less than r, and will generally be a given resistance, depending on the apparatus we are employing. If B is given, differentiating the expression for q with respect to C, and equating to zero, we obtain

$$C=\sqrt{\frac{BD(G+D)(\rho+B)}{(G+B)(\rho+D)}}.$$

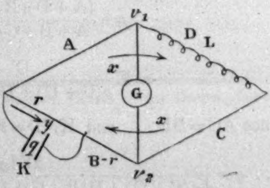
If ρ is small compared to the other resistances, this gives

$$C=B\sqrt{\frac{G+D}{G+B}}.$$

This method can be made much more sensitive by adopting the principle employed by Professors Ayrton and Perry in their admirable instrument the Secohmmeter. A commutator is introduced into the battery and galvanometer-circuits, which by its revolution puts on the battery, the galvanometer-circuit being closed, breaks the latter, or short-circuits the galvanometer and then breaks the battery-circuit, afterwards closing the galvanometer-circuit, repeating this cycle of operations during each revolution. In this method neither the speed of the commutator nor the fraction of the cycle during which the battery and galvanometer are on together need be known, as is the case with the secohmmeter.

To find the conditions under which a telephone may replace the galvanometer.

In order that the galvanometer may be replaced by a telephone it is necessary that the current through it at any moment shall be zero. Consequently v_1 must equal v_2 always. Therefore the resistance of G may be anything. Let it be infinite, and let x be the current in A, D, C, and $B-r$, y the current in r, and q the charge on the condenser at any moment t after the battery-circuit is broken.



Let x_0 be the current in D, y_0 the current in r, and q_0 the charge on the condenser at the moment of breaking the battery-circuit, and suppose the break is not prolonged by a spark at the contact.

Then

$$(C+D)x+L\frac{dx}{dt}=v_1-v_2=0, \quad . \quad . \quad . \quad . \quad (1)$$

$$(A+B-r)x-ry=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

$$x+y=-\frac{dq}{dt}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

From (2),

$$y = x\,\frac{A+B-r}{r};$$

and therefore from (3),

$$x\,\frac{A+B}{r} = -\frac{dq}{dt}. \qquad \qquad (4)$$

Now from (1),

$$\frac{dx}{x} = -\frac{C+D}{L}\,dt,$$

$$\int_{x_0}^{x} \frac{dx}{x} = -\frac{C+D}{L}\int_0^t dt;$$

or

$$\log\frac{x}{x_0} = -\frac{C+D}{L}\,t,$$

$$x = x_0 e^{-\frac{C+D}{L}t}.$$

Again, since the resistance of G may be anything, let it equal zero; then the condenser is discharging through a resistance equal to

$$\frac{r(A+B-r)}{A+B} = \frac{r\{B(C+D)-Cr\}}{B(C+D)},$$

since

$$AC = BD.$$

Therefore

$$q = q_0 e^{-\frac{t}{K\frac{r\{B(C+D)-Cr\}}{B(C+D)}}} = q_0 e^{-\frac{t}{K}\cdot\frac{B(C+D)}{r\{B(C+D)-Cr\}}},$$

and

$$-\frac{dq}{dt} = q_0\,\frac{B(C+D)}{Kr\{B(C+D)-Cr\}}\,e^{-\frac{t}{K}\cdot\frac{B(C+D)}{r\{B(C+D)-Cr\}}}.$$

Also

$$q_0 = Ky_0 r \quad \text{and} \quad \frac{A+B}{r} = \frac{B(C+D)}{Cr}.$$

Substituting these values in (4), we obtain

$$x_0\,\frac{B(C+D)}{Cr}\,e^{-\frac{C+D}{L}t} = y_0\cdot\frac{B(C+D)}{B(C+D)-Cr}\cdot e^{-\frac{B(C+D)}{Kr\{B(C+D)-Cr\}}t}.$$

Also

$$\frac{x_0}{y_0}=\frac{C}{D};$$

therefore

$$\frac{1}{r}e^{-\frac{C+D}{L}t}=\frac{D}{B(C+D)-Cr}e^{-\frac{B(C+D)}{Kr\{B(C+D)-Cr\}}t}.$$

This equation must hold for every value of t; hence

$$\frac{1}{r}=\frac{D}{B(C+D)-Cr}, \quad . \quad . \quad . \quad . \quad (A)$$

and

$$\frac{C+D}{L}=\frac{B(C+D)}{Kr\{B(C+D)-Cr\}}. \quad . \quad . \quad . \quad (B)$$

From (A),

$$rD=B(C+D)-Cr,$$

$$r(C+D)=B(C+D);$$

$$\therefore\ r=B \quad \text{or} \quad C=-D.$$

From (B),

$$L=\frac{Kr\{B(C+D)-Cr\}}{B}$$

$$=Kr\left(C+D-r\frac{C}{B}\right).$$

If $r=B$, $\qquad L=KBD.$

If $C=-D$, $\qquad L=Kr^2\frac{D}{B}.$

As $r=B$ is the only solution possible, the method can only be used with a telephone under the conditions given by Maxwell, and is impracticable on account of the trouble met with in obtaining the double adjustment.

VII. *On Evaporation and Dissociation.*—Part VI. (*continued*). *On the Continuous Change from the Gaseous to the Liquid State at all Temperatures. By* William Ramsay, *Ph.D.*, *and* Sydney Young, *D.Sc.*[*]

[Plates I.–III.]

The following pages give a further proof of the correctness of the relation $p=bt-a$, where $v=$ constant, applicable both to gases and liquids. The data for methyl alcohol apply solely to the gaseous state, for the very high pressures which its vapour exerts precluded measurements at temperatures above its critical point. With ethyl alcohol the determinations of the compressibility of the liquid are more complete than with ether; the experimental observations in the neighbourhood of the critical volume are, however, not very numerous, for the highest temperature for which an isothermal was constructed is 246°, the critical temperature being 243°·1. The values of a and b at volumes near the critical are consequently somewhat uncertain. The data for the gaseous condition are, however, pretty full. We have also a considerable number of data for acetic acid (Trans. Chem. Soc. 1886, p. 790). Here the temperature at which the highest isothermal was measured was the highest conveniently attainable by our method, viz. 280°. But as the critical temperature lies much higher, the pressures were in no case very great. The behaviour of acetic acid, however, contrasted with that of the alcohols, ether, and carbon dioxide is very striking. The equation $p=bt-a$ does not apply; in other words lines of equal volume are not straight, but are curves of double flexure. We shall consider the meaning of this peculiarity after adducing data.

1. *Methyl Alcohol.*—The data are at present in the hands of the Royal Society. The values of b were, as before, determined by reading points on the isothermal curves at equal volumes; constructing isochors graphically, and having thus obtained approximate values of b, these were smoothed by

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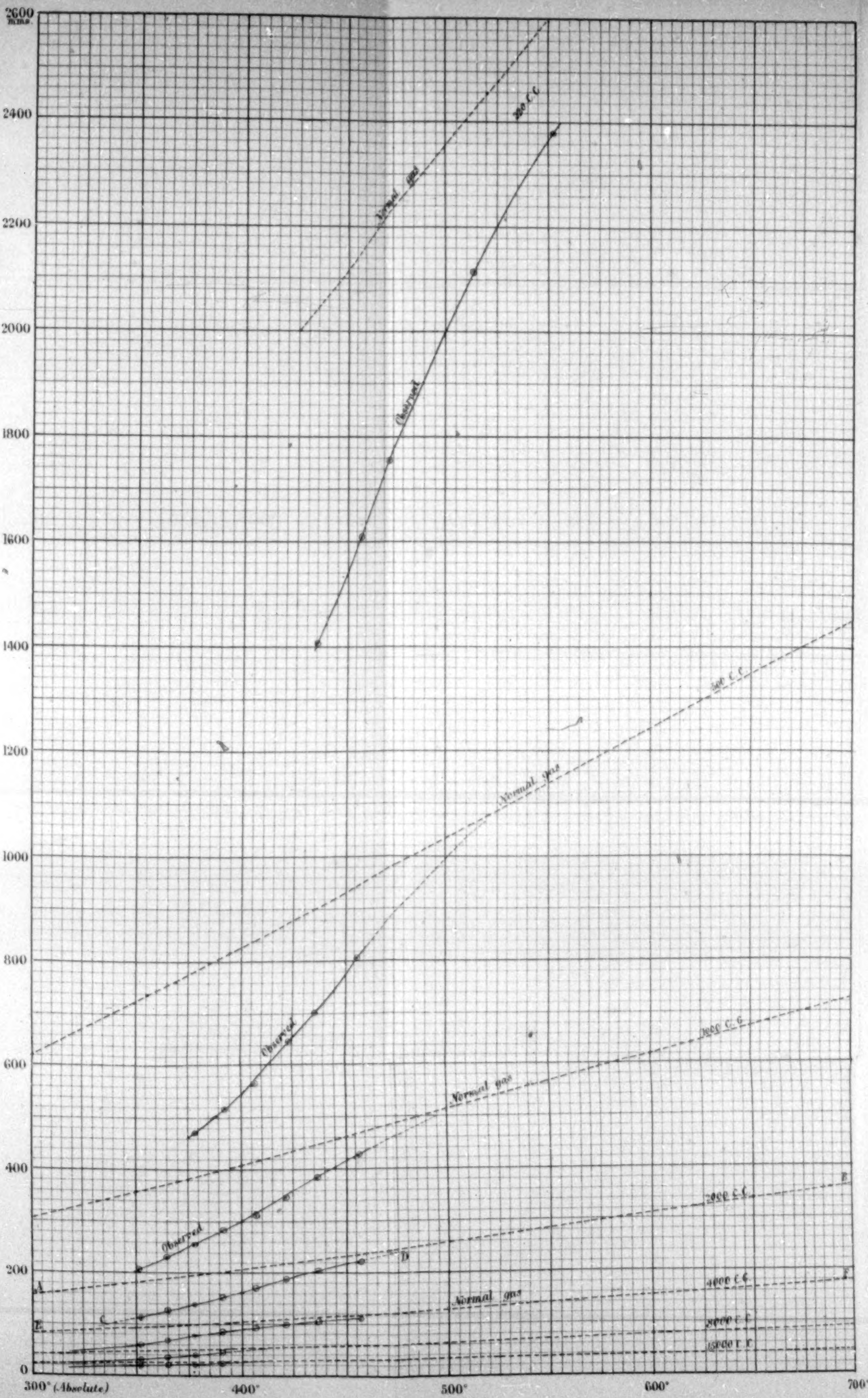
[*] Read April 23, 1887.

plotting them as abscissæ, the ordinates being the reciprocals of the volumes. The values of *b* given in the Table which follows were those read from this curve. The values of *a* were calculated from the equation $a = bt - p$, the mean value obtained from all the readings at each volume being taken as correct.

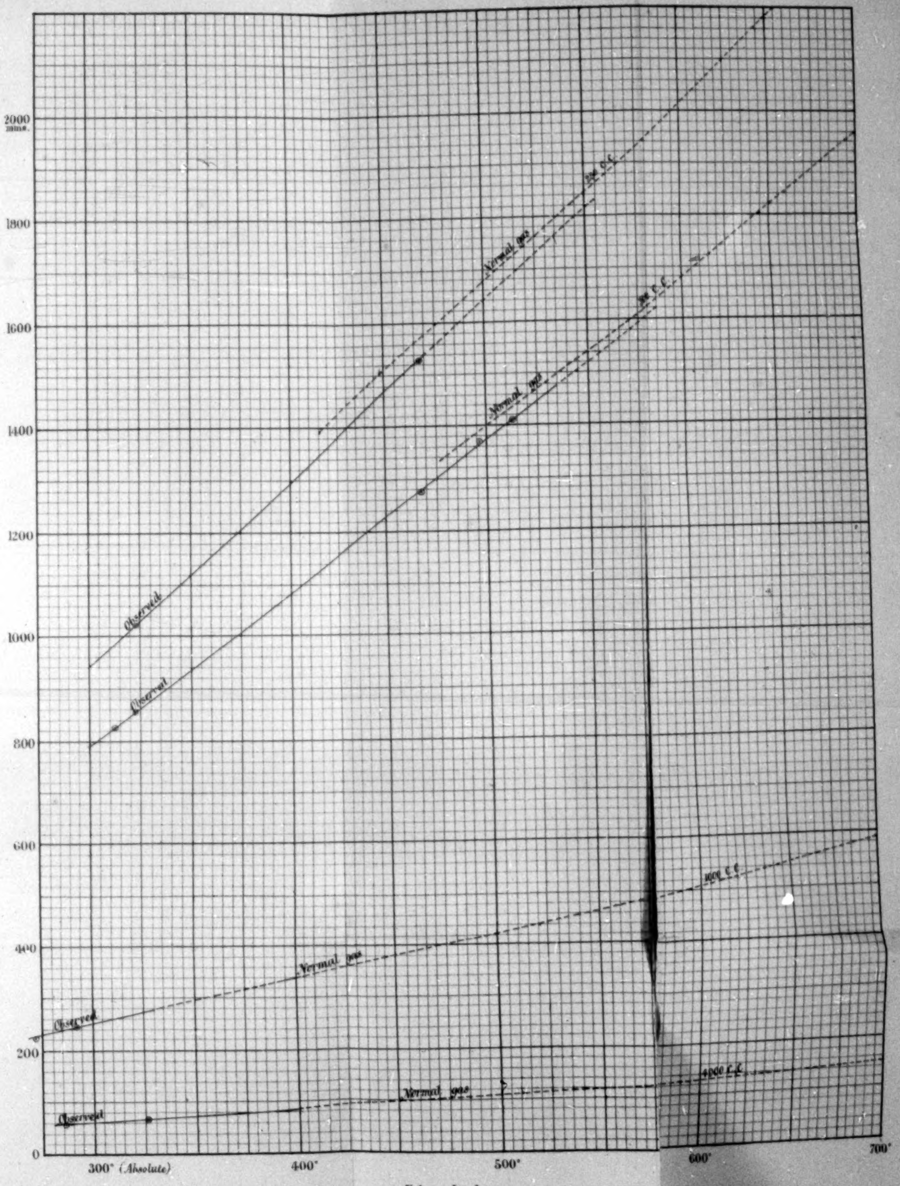
TABLE I.

Vol.	*b.*	log *b.*	*a.*
c. c. per gram.			
7	626·5	2·79692	263430
8	569·0	2·75511	235370
9	509·0	2·70672	206290
10	452·5	2·65562	179090
11	405·3	2·60778	156660
12	365·0	2·56229	137810
14	299·0	2·47567	107460
16	248·5	2·39533	84730
18	211·0	2·32428	68364
20	184·8	2·26670	57474
25	139·8	2·14551	39724
30	112·5	2·05115	29834
40	80·0	1·90309	18913
50	62·0	1·79239	13401
70	43·5	1·63849	8631
100	26·95	1·43056	4092
135	18·95	1·27761	2398
170	14·50	1·06137	1624
200	12·00	1·07918	1187
240	9·65	0·98453	794
280	8·00	0·90309	455
340	6·30	0·79934	307
400	5·15	0·71181	159
450	4·57	0·65992	130

The following Table gives the pressures read from the isothermals from which these values of *a* and *b* were obtained; and we have added, for the sake of comparison, the pressures recalculated by help of these values.



Acetic Acid Isochors.



Ether Isochors.

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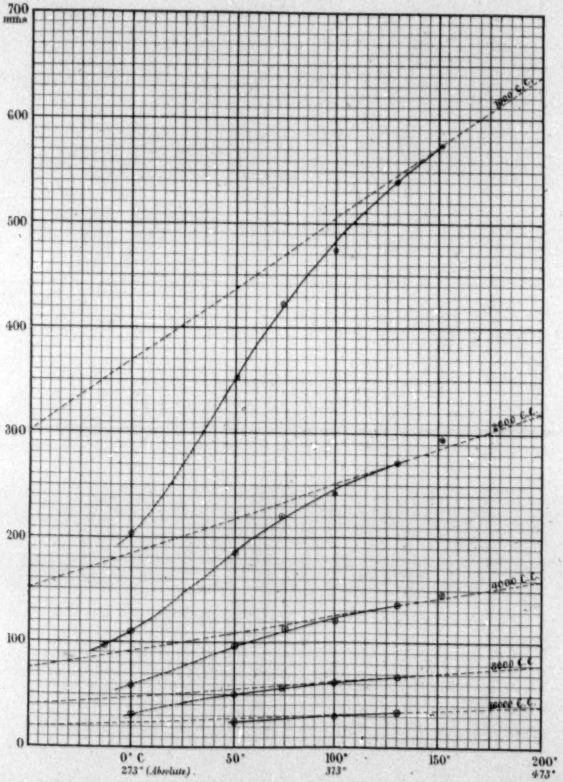
TABLE II.

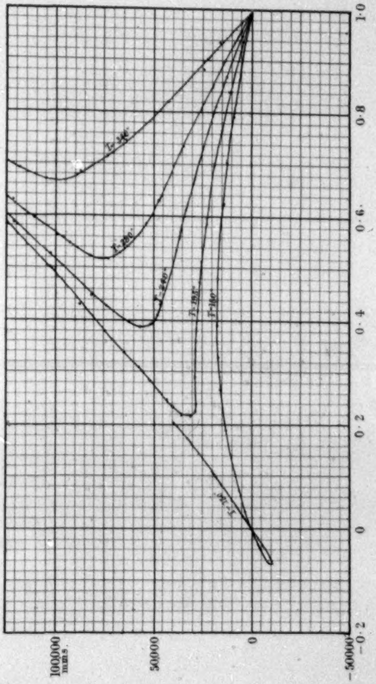
Vol. c. c. per gram	Temperatures.															
	240°.		239°·5.		239°.		238°.		236°.		234°.		232°.		230°.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
7	58220	57970	57700	57650	57280	57340	56660	56710	55250	55460	54200	54210				
8	56660	56530	56260	56240	55930	55960	55330	55390	54050	54250	53190	53110	52060	51570		
9	54990	54830	54580	54570	54240	54320	53790	53810	52640	52790	51910	51780	50800	50760	49630	49740
10	53200	53040	52860	52810	52600	52590	52040	52140	51180	51240	50390	50330	49430	49420	48370	48520
11	51390	51260	51160	51060	50870	50860	50320	50450	49640	49640	48780	48830	48000	48020	47040	47210
12	49450	49430	49380	49250	49150	49070	48610	48700	48070	47980	47190	47250	46530	46510	45710	45790
14	45980	45930			45670	45630	45260	45330	44800	44730	44150	44130	43620	43530	42930	42940
16	42800	42750			42570	42500	42190	42260	41780	41760	41250	41260	40840	40760	40200	40270
18	39920	39876			39660	39666	39400	39456	39000	39036	38600	38616	38250	38186	37740	37766
20	37360	37328			37110	37143	36900	36958	36600	36589	36205	36220	35940	35849	35400	35481
25	32000	31994			31810	31854	31670	31714	31440	31435	31120	31156	30900	30876	30600	30596
30	27845	27878			27805	27766	27630	27653	27420	27429	27200	27204	27000	26978	26795	26753
40	22065	22127			22000	22047	21930	21967	21820	21807	21650	21647	21510	21487	21375	21327
50	18380	18405													17820	17785
70	13590	13685													13210	13250
100	9690	9733													9460	9464
135	7300	7323													7186	7134
170	5800	5815													5679	5670
200	4960	4969													4856	4849
240	4153	4156													4064	4060
280	3558	3559													3494	3478
340	2921	2925													2873	2862
400	2491	2483													2442	2431
450	2211	2214													2178	2169

Table II. (*continued*).

Vol. c. c. per gram.	Temperatures.													
	225°.		220°.		200°.		180°.		160°.		120°.		100°.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
10	46260	46260												
11	45280	45180												
12	43980	43960	42000	42140										
14	41400	41440	39810	39950										
16	38990	39020	37740	37780										
18	36740	36716	35660	35656										
20	34600	34556	33660	33633										
25	29970	29897	29210	29198										
30	26200	26191	25700	25629	23790	23379	20600	21129						
40	20960	20927	20620	20527	19060	18927	17140	17327						
50					15980	15925	14600	14685						
70					11980	11945	11140	11075	10240	10205				
100					8640	8655	8200	8117	7605	7577				
135					6520	6565			5830	5808				
170					5221	5235			4696	4655	4056	4075		
200					4467	4489			4040	4009	3522	3529		
240					3748	3770			3404	3375	2999	2999		
280					3229	3239			2939	2919	2605	2599	2405	2439
340					2670	2673			2435	2421	2172	2169	2022	2043
400					2274	2277			2079	2071	1863	1865	1740	1762
450					2024	2030			1860	1849	1665	1666	1565	1575



Nitric Peroxide Isochors.



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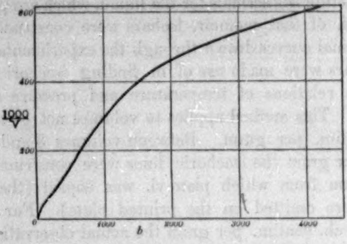
It is evident that the agreement here is very satisfactory, and that any differences are well within the limits of experimental error, and of error in reading from curves.

2. *Ethyl Alcohol.*—The data for ethyl alcohol are published in the 'Philosophical Transactions,' 1886, part i. p. 123. In constructing isothermals for the liquid, which are published in plate iii. of that memoir, isobars were constructed from the isothermal curves drawn through the experimental points; these isobars were made use of in finding isochoric lines at which the relations of temperature and pressure could be compared. This method applies to volumes not greater than 3 cub. centim. per gram. Between volumes 3 and 12 cub. centim. per gram the isochoric lines were constructed from the diagram from which plate vi. was copied (the smaller divisions are omitted on the printed plate). For volumes above 12 cub. centim. per gram the actual observations were made use of which are detailed on pp. 144–151; the slight alteration in volume caused by the expansion of the measuring-tube was here neglected; at the utmost the error introduced is 0·15 per cent. of the total volume.

The only satisfactory manner of comparing the found and calculated pressures for liquid alcohol is to reproduce the diagram showing the compressibility of the liquid, denoting the calculated pressures by crosses, the actual experimental observations being represented by circles. Although there is apparently only one volume at which a comparison is possible on each of the lower isotherms, yet it must be remembered that the values of a and b are deduced from the isobaric curves, to construct which numerous points on each isotherm were made use of.

Table III. on page 39 gives the values of a and b for definite volumes. These were read from three overlapping curves, on which values of b were abscissæ, the reciprocals of the volumes being ordinates; the actual values of b in the region of the critical volume were, for reasons previously given, interpolated. The form of this curve is remarkable. It is equivalent to the first term in Clausius's and Van der Waals's formulæ, $\frac{\mathrm{RT}}{(v-\beta)}$; but it is evident that its form cannot be represented so simply. Its general form is represented in the accompanying woodcut.

The same general form is to be noticed in the case of ether. The curve obtained in plotting *a* against volume is of similar form.

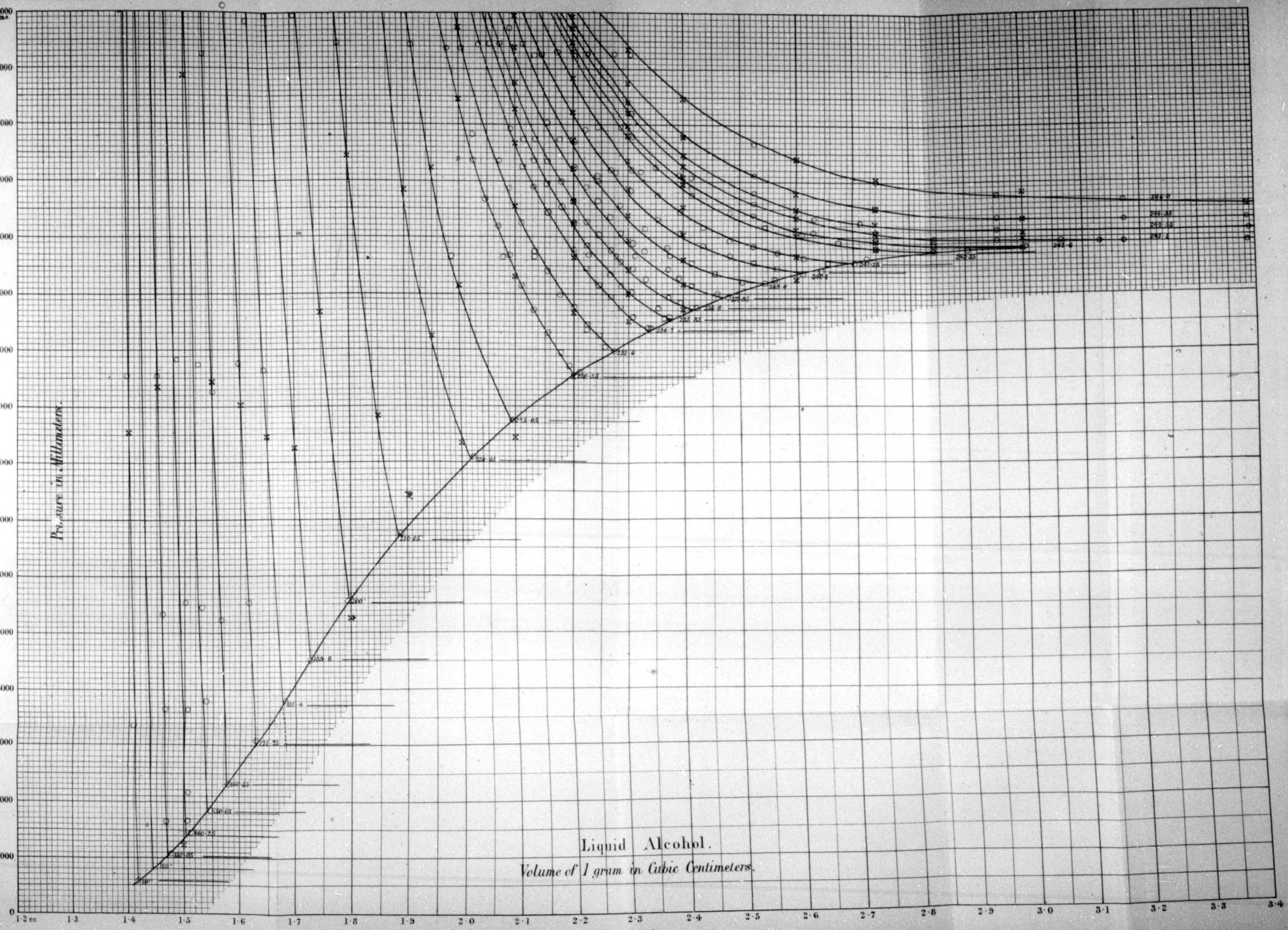


The comparison of actual and calculated results at volumes below 3 cub. centim. per gram is difficult. As it is most important to show the truth of the equation $p = bt - a$ for liquids, we have decided, after due consideration, to reproduce the isothermals exhibiting compressibility, inserting crosses (×) to denote the pressures calculated by means of the values of *b* and *a* given in the preceding Table. It will be noticed that these crosses are often more in concordance with the actual observations than are the lines drawn before we had discovered this relation. These are shown on Plate I.

Between volumes 3 and 12 cub. centim. per gram we give a table of comparison (Table IV.).

For higher volumes a comparison is made with the results of actual experiment. The volume at the mean temperature is given in each case, although at the extreme temperatures the divergence in some cases amounts to 0·15 per cent. (Table V.).

The values of *a* and *b* given are the best available from the determinations; but as these are few at volumes near the critical volume, the range of temperature being too small to permit of ascertaining the values with accuracy, no great dependence can be placed on them. This is proved by the fact that at 220°, if the areas included between the vapour-pressure line and the serpentine curve are made equal, the vapour-pressure shows an error of about 3 per cent. We do not regard this as in any way opposed to our conclusions; for while with ether about the critical volume, the range of temperature from which the values of *b* and *a* were calculated



Liquid Alcohol.

Volume of 1 gram in Cubic Centimeters.

Mintern Bros. lith.

amounted to 55 degrees, and while at volumes slightly greater measurements up to 85 degrees were made use of, with alcohol the total range of temperature was only 3 degrees at corresponding volumes. They cannot, therefore, have at all the same claim to accuracy.

Acetic Acid.—The data are given in the Trans. Chem. Soc. 1886, p. 790. As in the original paper we have omitted to state the volumes of 1 gram, we give a Table (VI.) containing these values.

TABLE III.

Volume.	*b.*	log *b.*	*a.*
c. c. per gram.			
1·4	6295	3·79900	2376800
1·45	5725	3·75778	2270060
1·5	5250	3·72016	2164680
1·55	4800	3·68124	2041900
1·6	4410	3·64444	1925370
1·65	4075	3·61013	1817760
1·7	3750	3·57403	1702370
1·75	3490	3·54283	1608180
1·8	3240	3·51054	1511410
1·85	3000	3·47712	1413870
1·9	2795	3·44638	1327920
1·95	2600	3·41497	1242030
2·0	2435	3·38650	1168030
2·1	2150	3·33244	1037000
2·2	1940	3·28780	938360
2·3	1765	3·24674	854080
2·4	1640	3·21484	793230
2·6	1435	3·15685	691280
2·74	1330	3·12385	638080
3·0	1170	3·06819	556040
3·5	977	2·98989	456490
4·0	842	2·92531	386810
4·5	741	2·86982	334710
5	662	2·82086	294080
6	556	2·74507	240150
7	484	2·68484	204010
8	428·5	2·63195	176650
9	381	2·58092	153610
10	341·2	2·53301	134650
12	276·5	2·44169	104590
14·28	221	2·34439	79460
18·25	155	2·19033	50530
22·13	113·7	2·05576	33080
26·08	88·4	1·94645	23120
30·0	72·1	1·85793	17030
37·8	52·4	1·71933	10470
53·45	34·2	1·53403	5400
76·6	22·6	1·35411	2902
108·2	15·3	1·18469	1565
131·7	12·7	1·10382	1282
170·5	9·6	0·98227	886
484	3·3	0·51851	204

TABLE IV.

Vol.	Temperatures.																			
	246°.		244°·35.		243°·55.		243·1°.		242°.		238°.		236°.		234°.		232°.		230°.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read,	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
c. c. p.gr.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
3·5	50180	50580	49100	48950	48400	48060	47640	47730												
4·0	50000	50190	48960	48790	48270	48120	47610	47740												
4·5	49840	49870	48730	48640	48180	48050	47520	47720												
5·0	49480	49500	48350	48400	48110	47880	47300	47580	46970	46850										
6·0	48420	48410	47460	47490	46670	46800	46210	46190	45210	45080										
7·0	47060	47180	46320	46380	45760	45780	45270	45250	44350	44280	43400	43310								
8·0	45500	45740	44920	45030	44440	44500	43030	44030	43220	43170	42450	42310	41600	41460	40640	40600				
9·0	43860	44130	43350	43500	42930	43020	42550	42600	41880	41840	41210	41080	40470	40320	39690	39560	38860	38790	38050	38030
10·0	42130	42430	41680	41870	41350	41440	40990	41070	40420	40390	39800	39900	39150	39020	38480	38340	37810	37660	37110	36980
12·0	38700	38910	38320	38460	38050	38110	37780	37810	37270	37250	36780	36700	36250	36150	35710	35600	35130	35040	34550	34490

TABLE V.

Temperature.	Volumes.											
	14·28 c. c. per gram.		18·25 c. c. per gram.		22·13 c. c. per gram.		26·08 c. c. per gram:		30·0 c. c. per gram.		37·8 c. c. per gram.	
	Pressures.		Pressures.		Pressures.		Pressures.		Pressures.		Pressures.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
246°	34928	35240	29929	29915	25936	25931	......	......	20430	20390	16817	16726
244·35	34657	34870	29891	29658	25664	25651	......	......	20269	20270	16704	16639
243·1	34546	34600	29438	29465	25570	25600	......	......	20158	20180	16611	16573
242·0	34093	34360	29372	29295	25554	25476	......	......	20216	20101	16566	16516
241·0	34150	34130	29277	29139	25482	25361	......	......	20013	20029	16535	16463
240·0	33900	33910	29037	28985	25303	25248	......	......	19959	19957	16500	16410
239·0	33890	33690	28785	28830	25262	25134	......	......	20000	19885	16408	16359
238	33767	33470	28746	28675	24988	25021	......	......	19910	19813	16322	16306
237	33338	33250	28612	28520	24980	24907	......	......	19802	19748	16336	16254
236	33106	33030	28371	28365	24872	24794	......	......	19694	19669	16249	16202
234	32721	32590	27740	28055	24504	24566	......	......	19458	19524	16135	16097
232	32091	32140	27448	27745	24100	24338	......	......	19108	19380	15759	15992
230	31636	31700	27144	27435	23827	24111	21190	21345	19082	18975	15733	15887
225	30601	30600	26546	26660	23365	23543	20809	20903	18729	18875	15436	15625
220	29632	29490	......	......	22964	22975	......	......	18355	18515	15214	15363
210	......	......	24723	24335	21992	21837	19769	19577	17899	17794	14912	14839
200	......	......	......	......	20814	20700	......	......	17140	17073	14444	14315
190	......	......	......	......	......	......	17846	17809	16364	16352	13805	13791
180	......	......	......	......	......	......	......	......	......	......	13183	13267

Table V. (*continued*).

Temperature.	Volumes.											
	53·45 c. c. per gram.		76·6 c. c. per gram.		108·2 c. c. per gram.		131·7 c. c. per gram.		170·5 c. c. per gram.		484 c. c. per gram.	
	Pressures.		Pressures.		Pressures.		Pressures.		Pressures.		Pressures.	
	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.	Read.	Calc.
	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
246°	12381	12350	......	......	......	......	5276	5309	4091	4096	......	......
244·35	12313	12293	......	......	......	......	5259	5288	4090	4080	......	......
243·1	12260	12251	8793	8762	6332	6331	......	......	4053	4069	......	......
242·0	12238	12213	......	......	......	......	5216	5258	4062	4058	......	......
241·0	12207	12179	......	......	......	......	5212	5246	4055	4048	......	......
240·0	12188	12145	......	......	......	......	5198	5233	4046	4039	......	......
239·0	12161	12111	......	......	......	......	5185	5220	4029	4029	......	......
238	12087	12076	8673	8647	6274	6253	......	......	4030	4020	......	......
237	12094	12042	8676	8624	......	......	......	......	4022	4010	......	......
236	12073	12008	8669	8602	6249	6223	......	......	4029	4000	......	......
234	11969	11940	8628	8556	6210	6192	......	......	3999	3981	......	......
232	......	......	......	......	6089	6161	......	......	3935	3962	......	......
230	11635	11803	......	......	......	......	......	......	3898	3943	......	......
225	11488	11632	8251	8353	5968	6055	......	......	3863	3895	......	......
220	11366	11461	8168	8240	5916	5978	......	......	3823	3847	......	......
210	11193	11119	......	......	......	......	4873	4852	......	......	......	......
200	10863	10777	......	......	......	......	4758	4725	......	......	......	......
190	10553	10435	......	......	......	......	4647	4598	......	......	......	......
180	......	......	......	......	5400	5366	......	......	......	......	......	......
170	9704	9751	7131	7110	5261	5213	......	......	......	......	......	......
160	9254	9409	6915	6884	5105	5060	4272	4217	......	......	......	......
150	......	......	6632	6658	4933	4907	4128	4090	......	......	......	......
140	......	......	......	......	4749	4754	3978	3963	......	......	......	......
130	......	......	......	......	......	......	3838	3836	......	......	......	......
120	......	......	......	......	......	......	......	......	2942	2887	......	......
110	......	......	......	......	......	......	......	......	......	......	1062	1060
100	......	......	......	......	......	......	......	......	......	......	1026	1027
90	......	......	......	......	......	......	......	......	......	......	993	994

Table VI.

Temp. 50°.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
13·4	15100
15·4	13300
16·95	11979
18·2	10640
21·0	9325
24·15	8000
26·5	7101
30·0	6214
34·9	5329
51·35	3567

Temp. 78°·4.	
18·8	14550
19·3	14760
20·95	13320
21·1	13250
21·85	12430
22·85	11980
23·35	12000
23·45	11550
24·9	10660
27·0	9774
27·15	9760
27·3	9858
29·3	8868
31·8	7962
32·7	7833
35·85	7092
40·15	6217
42·05	6075
46·4	5326
52·9	4631
54·35	4441
63·2	3865
66·75	3553
71·7	3281
87·2	2670
91·2	2496
101·25	2220
106·6	2132
121·5	1826
124·65	1782
125·3	1767
141·3	1519
151·7	1421
157·75	1375
164·2	1306
172·25	1243
180·8	1175
191·75	1066

Temp. 92°.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
20·45	14650
24·55	11990
30·58	9325
39·35	7105
51·1	5329
66·45	4000
75·65	3515
96·8	2649
104·2	2440
135·8	1825
138·6	1792
170·5	1403
181·05	1327
234·9	982
268·4	843

Temp. 105°·1.	
21·85	15150
31·9	10210
53·6	5782
74·65	4005
83·45	3590
91·4	3110
106·3	2721
126·3	2220
150·4	1823
155·4	1778
186·2	1431
199·75	1334
232·6	1123
286·5	884·6
298·6	841·5
337·4	729·3
422·4	561·0
460·35	504·9

Temp. 117°·95.	
75·75	4488
85·35	3871
163·5	1868
210·45	1403
256·4	1121
328·6	841·5
386·1	701·3
467·1	561·0
601·8	420·8
632·5	392·7
676·8	364·7

Temp. 118°·2.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
24·1	15100
29·1	12430
36·65	9769
44·0	7984
55·3	6217
69·25	4885
84·05	4010
88·6	3782
121·5	2664
172·6	1781
224·2	1335
319·0	876·6
391·9	689·2
433·6	611·0

Temp. 132°·9.	
101·7	3591
132·7	2665
174·5	1969
214·9	1546
285·2	1122
360·4	851·3
470·7	617·1
700·2	391·3
747·6	364·7

Temp. 147°·6.	
88·5	4516
133·9	2805
181·6	2045
225·1	1627
263·5	1352
341·8	1010
427·9	785·4
507·0	645·2
602·5	533·0
729·7	420·8

Temp. 162°·5.	
95·9	4460
134·3	3142
183·8	2244
274·0	1459
373·1	1038
450·5	841·5

Table VI. (*continued*).

Temp. 162°·5 (*cont.*).	
Pressure.	Vol. of 1 gram.
millim.	c. c.
526·7	701·3
640·3	561·0
729·5	476·9

Temp. 184°·1.	
98·05	4654
140·7	3223
188·2	2384
242·9	1823
336·8	1290
425·7	1010
520·4	813·5
666·4	617·2
791·6	504·9

Temp. 162°·5.	
1413	217·1
1523	197·3
1616	184·1
1777	164·0
1907	150·5
2049	137·3
2128	130·6
2212	124·0
2291	117·4
2390	110·7
2456	104·1

Temp. 183°·7.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
1630	216·8
1764	197·0
1876	183·8
2066	163·8
2299	143·7
2599	123·8
2993	103·9
3511	84·17
3733	77·64

Temp. 200°.	
1780	216·8
1984	190·5
2341	157·1
2852	123·9
3679	90·75
4452	71·14
5178	58·15

Temp. 240°.	
2139	217·5
2389	191·1
2851	157·6
3512	124·2
4578	91·02
5612	71·36
6604	58·33
8043	45·27
9006	38·72
10295	32·16
11930	25·60

Temp. 279°·85.	
Pressure.	Vol. of 1 gram.
millim.	c. c.
20669	15·82
21043	15·34
21444	14·86
21762	14·38
22147	13·90
22565	13·42
22993	12·93
23383	12·44
23907	11·95
24075	11·70
24260	11·46

Temp. 280°·4.	
2400	217·5
2475	211·1
2556	204·5
2639	197·9
2731	191·3
2824	184·6
3284	157·7
4085	124·4
5383	91·12
6661	71·44
8742	51·87
11048	38·77
12742	32·20
15105	25·63
18496	19·04
20693	15·74
21848	14·42
23433	12·44

These numbers were mapped on curve-paper, and pressures were read from the curves, corresponding to even volumes.

TABLE VII.

Vol.	50°.	78°·4.	92°.	105°·1.	118°·1.	132°·9.	147°·6.	162°·5.	184°·1.
c. c.	mm.	mm.	mm.	mm.	mm.	mm.	mm.	m m .	mm.
15000	13·85	18·7	20·0	22·0	24·3				
8000	24·00	32·25	35·15	39·65	43·9				
4000	45·45	60·4	66·55	75·1	83 65	93·0	97·7	106·0	114·7
2000	......	111·0	124·9	140·3	155·0	171·5	185·5	204·5	221·5
1000	......	205·0	231·2	257·5	284·5	315·2	345·0	385·0	430·0
500	......	......	......	469·7	517·5	563·0	635·0	700·5	802·5

Table VII. (*continued*).

Vol.	162°·5.	183°·7.	200°.	240°.	280°.
c. c.	mm.	mm.	mm.	mm.	mm.
220 ...	1405	1610	1755	2115	2380
150 ...	1910	2225	2435	2985	3440
100 ..	2545	3080	3400	4235	4962
80 ...	......	3670	4055	5105	6030
60 ...	......	......	5057	6465	7750

Before discussing these results we give similar tables calculated and read from numbers given in the Messrs. Natansons's memoir on nitric peroxide (Wiedemann's *Annalen*, 1886, p. 606). As their results are stated as vapour-densities, we have converted their numbers into volumes of 1 gram.

TABLE VIII.

Pressure.	Vol. of 1 gram.
Temp. −12°·6.	
millim.	c. c,
115·4	1648
Temp. 0°.	
37·96	6235
86·57	2539
172·48	1208
250·66	808·1
Temp. 21°.	
491·60	479·8
516·96	453·0
556·50	419·4
619·17	360·6
Temp. 49°·7.	
26·8	15585
93·75	4144
182·7	2008
261·4	1354
497·75	651
Temp. 73°·7.	
millim.	c. c.
49·65	9214
64·75	7017
67·72	6754
107·47	4184
164·59	2701
302·04	1420
504·14	816·8
633·27	635·9
Temp. 99°·8.	
11·73	41542
23·22	20993
34·80	13929
57·35	8468
79·57	6067
89·67	5408
108·65	4454
116·58	4156
142·29	3402
202·24	2375
371·27	1277
520·98	900
658·31	705·5
675·38	687·7
732·51	629·7
Temp. 117°·91.	
58·24	8972·3
Temp. 129°·9.	
35·99	15076
66·94	8100
78·73	6892
104·77	5182
152·46	3568
169·71	3194
247·86	2186
297·95	1823
550·29	980·1
Temp. 151°·4.	
117·98	4869
475·41	1210
666·22	861·1

Those numbers, after mapping, gave the following pressures for even volumes :—

TABLE IX.

Volumes.	−12°·6.	0°.	49°·7.	73°·7.	99°·8.	129°·9.	151°·4.
c. c.	mm.	mm.	mm.	mm.	mm.	mm.	mm.
16000	......	......	26·3	......	30·0	34·5	
8000	......	30·7	50·8	56·5	61·2	68·8	
4000	......	58·5	96·2	112·5	121·2	136·0	146·3
2000	97·0	110·0	183·7	220·0	241·0	271·3	294·0
1000	......	204·2	353·0	422·5	473·5	540·0	573·5

On comparing the results for acetic acid and for nitric peroxide with those for methyl and ethyl alcohols, ether, and carbonic anhydride, the most noticeable feature is that the isochoric lines are not straight. If it is true that the limiting formulæ for acetic acid and for nitric peroxide are, respectively, $C_2H_4O_2$ and $C_4H_8O_4$, and NO_2 and N_2O_4*, then, on the supposition that the lower formulæ remain correct, and that the density remains constant, it is possible to calculate pressures and temperatures corresponding to given isochoric lines; these lines would be those of a perfect gas, straight, and radiating from absolute zero of temperature and pressure; and the isochoric line corresponding to a volume v of the simpler molecule $C_2H_4O_2$, or NO_2, will be the same as the isochoric line for the formula $C_4H_8O_4$ or N_2O_4, and the volume $2v$. Now as the density of the more complex molecule diminishes with rise of temperature and pressure, it is to be expected that the isochoric line should lie between these lines, and that it should coincide at high temperatures with the isochoric line v, and at low temperatures with the line $2v$.

This will be readily understood on reference to the accompanying plates. Plate I. represents isochoric lines for acetic acid, and Plate II. for nitric peroxide. Take the volume 2000 cub. centims. per gram. The line AB represents the relations of temperature to pressure on the assumption that the formula of acetic acid is $C_2H_4O_2$, and its corresponding density 30. The line EF represents similar relations on the assumption that the formula is $C_4H_8O_4$, and the density 60.

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* We must here direct attention to an arithmetical mistake in our former paper, "On the Nature of Liquids" (Proc. Phys. Soc. vol. viii. p. 136). The density of nitric peroxide at −12°·6 is stated to be 52·54, it should be 42·54; and the deductions from that erroneous statement must therefore be withdrawn.

This line, it will be seen, also corresponds to a volume of 4000 cub. centims. of gas of density 30. The line CD represents the observed relations of temperature to pressure for acetic acid when the volume is 2000 cub. centims. per gram. It is a curve of double flexure, which, if produced towards B, apparently might touch and become continuous with the line AB ; and if produced towards E might coincide with the line EF. For these large volumes of gas, it is almost certain that the density would become normal at a sufficiently high temperature ; but at low temperatures and pressures it cannot be proved, owing to condensation, whether the molecular formula would be $C_4H_8O_4$; that is, whether the line CD would coincide with the line EF.

On Plate I. are also represented similar relations for ether, at volumes of 4000 and 1000 cub. centims. per gram ; the points representing the observed relations of temperature to pressure fall on the isochoric lines ; that is to say, the divergence from Boyle's and Gay-Lussac's laws is too small to be detected by experiment. At smaller volumes, however (those at 300 and 250 cub. centims. per gram are shown), the line passing through the observed points falls below the theoretical line, but is not quite parallel to it. It follows therefore, that if the isochoric lines are perfectly straight, they would cut the normal isochoric lines at an extremely high temperature. The physical meaning of this behaviour is that, if the temperature of a gas, at constant volume, be raised sufficiently high, the density must equal and then fall below the normal. It is evident that this must be the case. For the pressure of a gas depends on the number of molecules present in unit volume, on the average velocity of each molecule, and on the number of impacts on unit area of the surface of the containing vessel, in unit time. With constant volume, since the mean distance between the molecules remains constant, the cohesion of the molecules is assumed to be constant. But the rise of pressure produced by rise of temperature of a theoretical gas is based on the assumption that each impact takes place at the centre of each molecule; that is, that the actual volume of the molecules themselves is *nil*. But as this is not the case, as impacts must take place at some distance from the centres of the molecules, they must necessarily be more frequent. The effect of cohesion is to reduce the pressure of the gas, by reducing the average velocity of the molecules, and this, for any given volume, by a constant amount. Hence, below a certain temperature, the pressure will be less than that of a normal gas, and if the temperature be reduced sufficiently, will become negative. With rise of temperature, the average velocity of each molecule will increase at the same rate as in the case of a theoretical gas; but the number of impacts, and, consequently, the pressure, will increase at a greater rate than if the gas were perfect; hence a temperature will ultimately be reached when the pressure is as much decreased by cohesion, as it is raised by the more frequent encounters of the molecules; and at that temperature the density of the gas will be normal. At still higher temperatures the pressure, and therefore the value of the expression $\frac{pt}{v}$, will be greater, and the vapour-density less, than that of a theoretical gas.

The formula which we find to represent the relations of temperature and pressure at constant volume, $p=vt-a$, represents these facts. The reciprocal of the vapour-density is the product of pressure and volume, divided by the product of the absolute temperature into half the molecular weight, or

$$\frac{1}{\text{vap. dens.}}=\frac{pv}{t}\times\frac{2}{\text{mol. wt.}}.$$

Now while at small volumes the rate of diminution of volume of a substance increases more and more slowly with rise of pressure, there is no limit conceivable to the pressure which may be applied. Hence the value of the expression $\frac{pv}{t}$ must ultimately be greater than unity. The relation of $\frac{pv}{t}$ to the pressure for ether has been calculated by means of our equation for a few isothermals, and is shown in the annexed figure. From the direction of the lines, it would appear that if produced to still higher pressures, the product pv would reach and exceed unity. This state has indeed been reached by Natterer in compressing the so-called permanent gases at temperatures far above the critical points.

It is possible, by means of our equation, to follow these lines into the unrealizable state, at low temperatures, where pressure becomes negative. An example is given at 150°. It is obvious that those isothermals which include negative pressures will intersect each other at the zero of pressure and $\frac{pv}{t}$, and will form loops in the negative region. This is shown on Pl. II.

It has recently been suggested by Wroblewski (*Wien. Monatsb. der Chemie*, 1886, p. 383), in a paper from the conclusions of which we differ in every point, that the minimum values of *pv* mapped against pressure form a curve continuous with the vapour-pressure curve. This is distinctly not the case. The curve is cut by the vapour-pressure curve at the critical point. It is, however, approximately continuous with the curve shown on plate ix. in our previous paper on this subject, representing the pressures corresponding to the inferior apices of the serpentine isothermals. The minimum product of pressure and volume probably does not occur at volumes corresponding to these apices, but at slightly lower volumes. We have proved this to be the case at high temperatures, and it is probably also the case at lower temperatures.

In conclusion, we should state that we have purposely omitted reference to the formulæ of Clausius, Van der Waals, and Sarrau. It appears to us that the relations of *a* and *b* in our formula to the volume is by no means so simple as that expressed by their formulæ.

VIII. *On the Production of Sudden Changes in the Torsion of a Wire by Change of Temperature.* *By* R. H. M. BOSANQUET, *St. John's College, Oxford*[*].

[Plates IV. & V.]

THE wire which forms the subject of the following observations is the finest platinum wire which can be procured; its diameter is about ·001 inch, and the length of the piece used was about 3 feet 6 inches. It originally formed the suspension of a galvanometer having a pair of nearly astatic needles about 7 inches long. The steel needles could be replaced by brass ones for the purpose of examining the properties of the suspension.

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[*] Read May 14, 1887.

During the summer no disturbances worth attention were noticed; but in the winter, when the use of the stove occasioned rapid changes of temperature, disturbances became evident. The brass needles were employed throughout the present investigation.

The tables and diagrams which follow, except the last, all refer to one and the same state of the wire, the suspended weight not having been lifted during their progress. The lifting of the weight entirely changes the condition of the wire so far as these phenomena are concerned. The last table and diagram exhibit a subsequent state in which the phenomena are less distinct and in reversed direction.

The angular position of the needles was read on a scale of degrees attached to the circular glass shade surrounding them. This position, which we may call the elongation, is the abscissa of the diagrams. The temperature was measured by a very sensitive thermometer with a long spiral bulb, hung up alongside of the wire. The temperature in degrees Fahrenheit forms the ordinate of the diagrams.

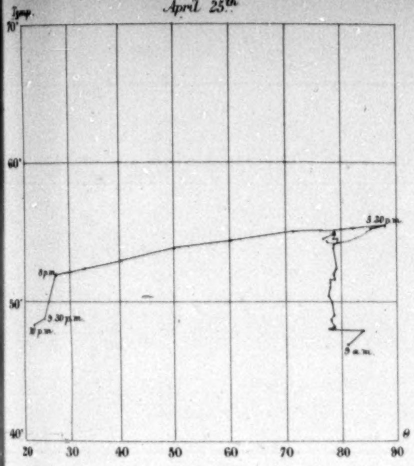
The principal effects are as follows:—The elongation varies between the limits of about 21° and 85°. Rising temperature drives it up to about 85°, where it remains while the temperature continues to rise. Falling temperature drives it down to about 21°, where it remains while the temperature continues to fall. The change of elongation is usually complete or nearly so by the time that the reversed change of temperature amounts to from 2° to 5°.

If the change of temperature is reversed as soon as the elongation reaches either of its limits, the line of the diagram returns along its original course: see diagram, April 27th. (The zigzags in the middle of this diagram arise from the rapid movement setting the needles in vibration.)

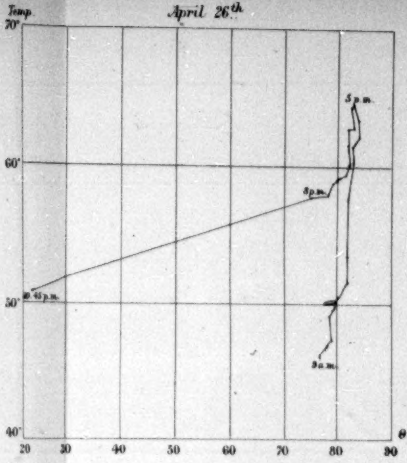
If the change of temperature is pushed in each direction after the limiting elongation is attained, we obtain a cycle of events which can be most simply represented by a parallelogram or series of parallelograms, such as the following:—

Here the vertical lines represent the limits of elongation, the one corresponding to rising temperature, the other to falling temperature. On reversal of the direction of change of temperature such lines as the sloping lines are described.

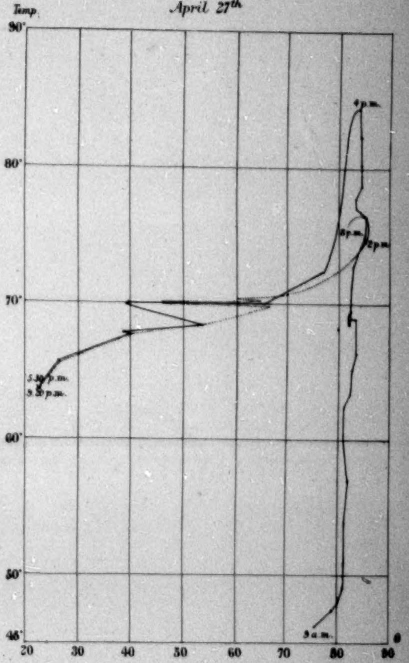
April 25th



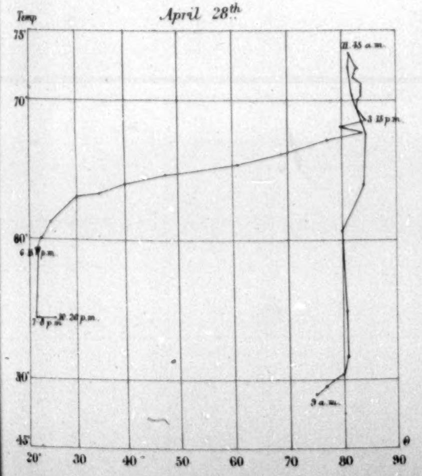
April 26th



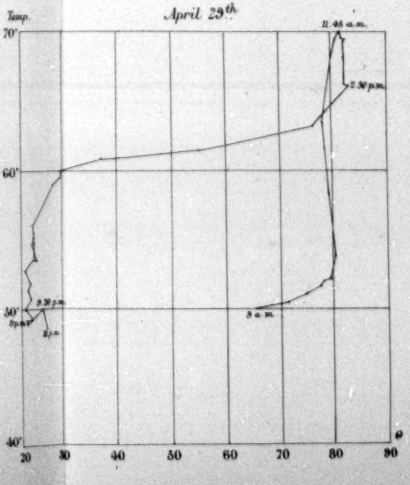
April 27th



April 28th

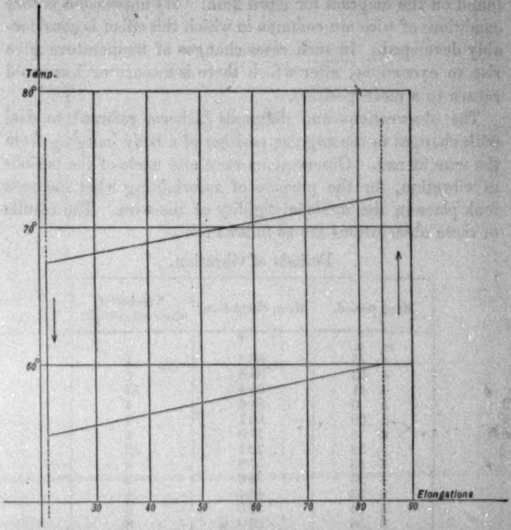


April 29th



Mintern Bros. lith.

The inclination of these is nearly constant, and they may lie at any height between the lines of limiting elongation.



The above figure is a typical representation of the character of the principal effect; but the lines are never really straight, and there are subsidiary effects, which, however, in the present case are small compared with that above described.

The tables and figures at the end of this paper explain themselves.

There was some difficulty in obtaining that part of the curve which corresponds to the lower horizontal line of the typical parallelogram, as the corresponding change of temperature usually took place in the small hours of the morning. The diagram May 4th–5th shows a case in which this part of the curve was obtained. This exhibits a subsidiary effect, which consists of a tendency to increase the elongation* in the neighbourhood of the lower limit when the temperature is constant. Another very clear case of this effect will be found on the diagram for April 28th. My impression is that conditions of wire are common in which this effect is considerably developed. In such cases changes of temperature give rise to excursions, after which there is a more or less rapid return to a mean position.

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* *I. e.* to return from the lower limit towards the mean position.

The observations and diagrams hitherto referred to deal with changes in the angular position of a body hanging from the wire at rest. Observations were also made of the periods of vibration, for the purpose of ascertaining what changes took place in the torsional rigidity of the wire. The results of these observations are as follows:—

Periods of vibration.

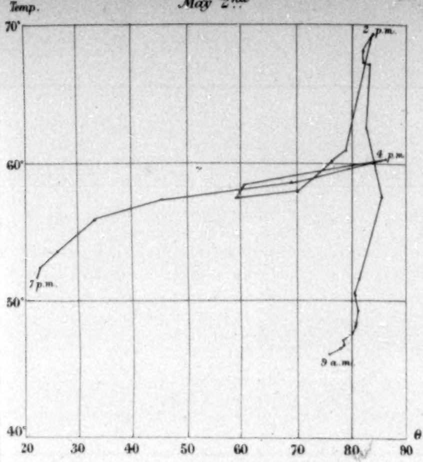
Mean period.	Mean elongation.	Number of observed periods.
m. s.		
4 45	89·2	4
4 53	88·5	4
4 41	85·4	13
5 0	84·6	4
5 39	83·7	4
6 3	77·0	4
5 52	72·1	12
6 33	57·9	4
4 23	54·6	2
5 33	46·4	4
5 16	37·0	2
3 56	35·0	8
3 4	32·3	2
2 29	28·6	2
2 40	27·5	4

It appears from these numbers that the period of vibration is least when the elongation is in the neighbourhood of the lower limit, increases rapidly as the limit is departed from up to a maximum value, and diminishes again slightly as the upper limit is approached and attained; that is to say, the torsion force has

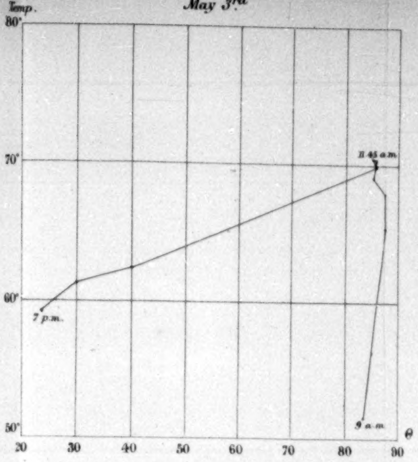
a maximum at the lower limit of elongation;
a minimum between the limits;
and an increase up to the upper limit.

The observations for the period are necessarily complicated by the changes in the position of rest, and they are not of great accuracy or consistency; but there can be no doubt that the course of the values is as above described.

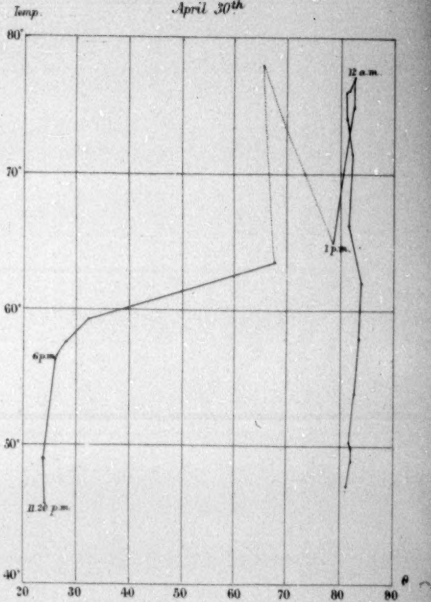
May 2nd



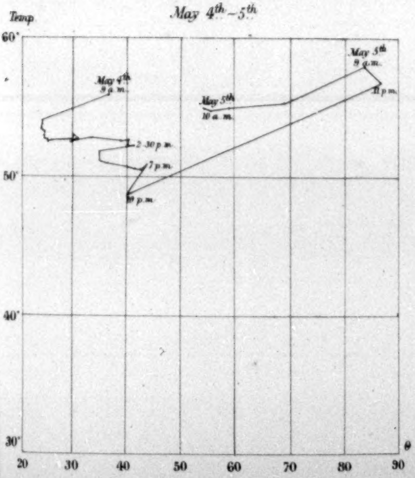
May 3rd



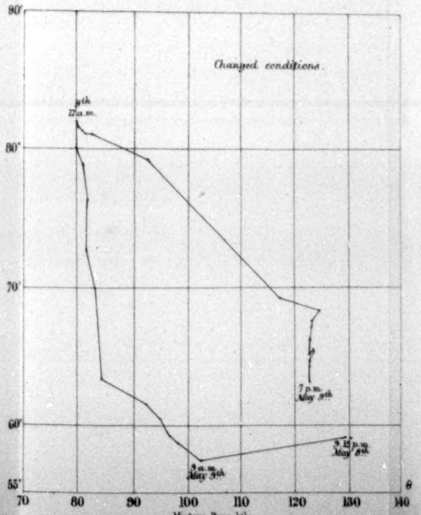
April 30th



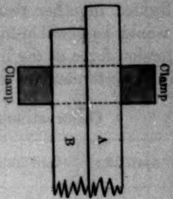
May 4th – 5th

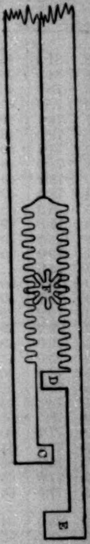


May 8th – 9th



Mintern Bros. lith.





It only remains to suggest what may be possible in the way of explanation of these phenomena. It appears to me that the only way of throwing any light on them is to imagine a mechanism capable of producing angular changes similar to those in question under the influence of changes of temperature.

A B are materials differently expansible by heat, capable of shifting, but with difficulty, through the clamp which binds them together.

Suppose C to be free between D and E; then any change of temperature will cause C to shift its position between D and E, and the pinion F will be turned round by the racks between which it lies. As soon as C comes up either against D or E the rotation of F is stopped, and the bars are compelled to shift in the clamp, if there is further change of temperature.

This arrangement exactly reproduces the typical changes observed in the present experiments. It is intended to serve the purpose of a formula.

The suggestion which we obtain from the above mechanism is that of loose contact between the parts involved, in this case probably between the molecules of the metal. As these peculiarities appear to originate in the extension of the wire by the suspended weight, it is possible that a partial separation of the molecules involving loose contact may arise out of this extension.

In any such case the molecules would bed against each other at the opposite parts of the loose contact, under the conditions which would give rise to either of the limiting elongations in the above experiments. The whole framework would be much stiffer when the loose contacts were thus bedded together than when they were free. In this way it would be possible to account for the increased values of the torsion force at the limiting elongations, and its lesser values in the intermediate region.

Observations of Temperature and Elongation.

Time.	April 25th.		April 26th.		April 27th.		April 28th.		April 29th.		April 30th.	
	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.
9 A.M.	46°·8	81·5	45°·7	77·0	46°·3	75·5	48°·8	75·0	50°·0	65·7	47°·2	81·5
9.15	47·8	84·0	46·0	77·0	47·5	79·2	49·5	77·0	50·4	71·3	49·0	82·0
9.30	48·0	78·0	46·7	78·5	48·1	80·2	50·5	80·0	51·0	75·0	49·8	82·0
9.45	48·2	79·0	47·2	78·5	49·2	81·0	51·7	80·5	51·5	77·5	50·3	81·5
10.0	48·7	78·4	49·0	79·0	50·9	81·0	55·0	80·5	51·7	78·3	54·0	82·5
10.15	49·0	79·0	48·7	79·2	54·0	81·0	60·6	80·0	52·1	79·5	58·0	83·3
10.30	49·8	78·3	50·3	79·8	57·3	81·8	63·8	83·5	53·8	80·5	62·0	84·0
10.45	50·5	78·0	49·5	79·2	60·2	81·2	67·5	84·0	58·4	79·5	66·2	81·5
11.0	51·0	78·3	49·7	78·8	62·5	81·3	70·6	81·6	63·5	78·3	71·5	82·0
11.15	51·4	78·3	49·5	79·2	63·3	82·2	72·4	81·2	68·3	80·2	74·1	80·8
11.30	51·6	78·3	49·6	79·2	65·2	82·5	73·3	81·2	69·6	80·5	76·2	81·0
11.45	51·6	78·8	49·7	79·4	66·5	83·0	73·5	81·5	70·0	81·5	76·2	81·2
12.0	51·8	78·8	50·2	79·5	68·9	83·0	72·8	81·5	69·8	81·5	77·0	82·7
12.15	52·4	79·0	50·3	79·7	69·4	82·3	72·4	82·5	69·6	81·5	76·3	82·5
12.30	53·8	78·5	...	...	69·0	82·3	72·0	82·0	69·5	82·5	75·0	82·3
12.45	54·0	78·7	50·2	79·3	69·2	82·5	71·6	82·0	69·0	82·0	65·0	78·5
1 P.M.	54·0	78·7	50·5	79·8	68·6	82·0	71·3	82·5	68·8	82·0	78·0	64·6
2.0	54·2	79·2	49·7	78·0	76·5	85·0	70·7	83·0	66·8	82·0		
2.15	54·2	79·2	49·7	78·3	77·0	83·5	70·4	83·0	66·2	82·2		
2.30	54·4	79·0	49·6	78·0	77·7	83·5	70·0	82·5	66·0	83·0		
2.45	54·4	79·0	49·8	78·5	78·7	84·5	69·2	82·5	63·2	76·0		
3.0	54·5	78·8	50·6	80·2	80·3	84·5	68·6	84·0	61·4	50·5		
3.15	54·5	78·8	51·4	82·0	82·4	84·5	68·4	83·0	60·7	37·0		
3.30	54·7	78·8	53·4	82·0	84·3	84·0	68·1	79·8	59·9	29·6		
3.45	55·0	78·5	57·6	82·3	84·4	84·0	67·6	83·5	59·6	29·5	59·3	32·0
4.0	55·0	77·7	60·3	83·0	84·6	84·0	67·0	77·5	59·0	27·5		
4.15	54·5	77·0	61·2	83·0	72·4	77·0	66·2	68·5	56·1	22·5		
4.30	54·5	76·7	62·0	84·0	70·0	65·0	65·5	61·0	55·5	22·5		
4.45	54·3	77·5	63·1	84·0	68·0	33·0	64·8	47·7	55·2	22·5		
5.0	54·3	77·5	64·2	83·3	66·6	26·3	64·3	40·0	54·6	22·0		
5.15	...	...	...	...	...	...	63·7	34·5				
5.30	55·3	85·0	64·0	83·2	64·0	22·5	63·3	30·4	53·6	23·0		
5.45	55·5	88·0	62·6	83·2	66·0	27·0	61·4	24·3	53·6	22·0		
6.0	55·2	82·0	62·6	82·3	68·5	36·7	60·1	22·0	54·8	22·5		
6.15	55·0	76·0	61·2	82·3	68·3	36·8	59·0	21·0	52·8	20·8	56·5	25·5
6.30	55·0	71·3	60·7	82·2	69·5	60·0	59·6	21·8	51·8	21·7		
6.45	54·4	60·5	60·1	82·4	70·7	70·0	59·1	21·3	51·3	21·5		
7.0	53·7	50·3	59·5	82·0	74·5	85·0	56·0	21·3	50·7	22·0		
7.15	53·0	40·3	59·0	80·0	76·1	85·0	54·8	21·0	50·0	20·8		
7.30	52·5	33·0	58·7	79·2	76·5	84·3	54·6	21·0	49·2	22·5		
7.45	52·2	29·5	58·1	78·5	76·0	83·5	54·6	21·5	49·0	21·0		
8.0	52·0	27·0	57·8	75·0	75·7	82·0	54·6	21·5	48·7	21·0		

Time.	May 2nd.		May 3rd.		May 4th.		May 5th.		May 7th.		May 9th.	
	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.	Temp.	Elongation.
9 A.M.	46°·2	75·5	51°·7	83·0	56°·0	36·5	58°·0	83·0	50°·0	122·5	57°·4	102·5
9.15	46·5	77·5	...	...	...	...	55·3	69·0	50·2	122·0	58·7	98·0
9.30	46·7	78·5	...	...	54·1	23·5	55·0	59·0	50·5	119·8	59·2	96·8
9.45	47·0	78·5	...	...	53·8	23·2	54·9	55·5	51·8	115·0	60·5	95·0
10.0	47·6	80·0	...	...	53·5	23·0	...	...	52·3	106·0	61·5	92·0
10.15	48·2	80·5	...	...	53·3	23·5	...	...	53·5	101·0	64·3	84·5
10.30	49·3	81·0	...	...	53·0	23·5	...	...	56·8	90·0	70·0	83·0
10.45	50·4	80·5	65·3	87·5	52·8	23·5	...	...	...	...	72·7	81·5
11.0	51·5	81·0	68·0	87·0	52·8	24·0	...	...	...	...	76·2	81·8
11.15	...	...	69·0	85·0	52·6	24·5	...	...	69·8	78·5	78·8	80·8
11.30	57·5	85·0	70·2	86·0	52·7	26·0	...	...	71·5	80·7	80·2	80·0
11.45	...	...	70·4	85·0	52·7	29·0	...	...	73·5	79·0	82·0	80·0
12.0	62·5	82·0	...	...	52·7	30·5	...	...	65·1	82·5	82·0	80·3
12.15	...	...	...	...	53·0	30·0	...	...	63·3	87·5	81·7	80·5
12.30	67·0	83·5	...	...	52·7	30·0	...	...	61·5	94·5	81·5	81·0
12.45	67·2	82·0	...	...	52·6	29·0	...	...	60·7	96·5	81·2	81·5
1 P.M.	68·0	82·0	...	...	52·8	33·5	...	...	61·2	98·5	81·0	82·5
2.0	69·2	83·7	69·7	86·0	52·6	38·5	...	...	...	...	79·3	92·0
2.15	60·8	79·0	...	...	52·6	39·5	...	...	...	...	69·2	117·5
2.30	60·2	76·5	...	...	52·7	40·5	...	...	...	...	68·2	124·5
2.45	57·8	70·0	...	...	52·2	39·5	...	...	...	...	67·4	123·0
3.0	...	...	...	...	52·2	41·0	...	...	...	...	67·3	123·0
3.15	57·5	59·0	...	...	...	...	...	...	...	...	67·0	123·0
3.30	58·4	60·5	...	...	...	...	...	...	...	...	66·0	122·8
3.45	...	...	...	...	51·8	34·5	...	...	...	...	65·1	122·8
4.0	60·3	86·0	...	...	...	...	...	...	...	...	65·0	123·0
4.15	...	...	...	...	...	...	...	...	...	...	65·0	123·0
4.30	58·5	69·0	...	...	51·0	34·5	...	...	...	...	65·5	123·5
4.45	...	...	...	...	...	...	...	...	...	...	64·5	123·0
5.0	57·5	45·0	62·5	40·0	50·8	36·0	...	...	...	...	65·0	123·0
5.15												
5.30	56·0	33·0	61·5	30·0	50·7	40·0	...	...	...	...	64·3	123·0
5.45	...	...	...	...	...	...	...	...	...	...	64·3	123·0
6.0	53·6	26·0	...	...	50·5	41·5	...	...	...	...	65·0	123·0
6.15	...	...	...	...	...	...	...	...	...	...	63·5	123·0
6.30	52·5	22·7	...	...	50·5	42·5	...	...	...	...	63·5	123·0
6.45	...	...	...	...	...	...	...	...	59·7	121·5	63·0	123·0
7.0	51·8	22·0	59·0	23·0	50·8	43·5	...	...	...	...	62·8	123·0

After May 5th the wire was readjusted, and the subsequent readings refer to an altered condition of the wire.

It would be easy to imagine several types of framework with loose contacts which would be capable of realizing the phenomena; but at present evidence in support of any particular arrangement would be wanting.

Supplementary Observations.

Date.	Time.	Temp.	Elongation.
April 25	9.30 P.M.	49°·0	24·5
„ 25............	10.0	48·5	23·0
„ 26............	10.45	50·8	22·5
„ 27...	9.20	63·7	23·0
„ 28............	10·20	54·5	25·0
„ 29............	9.20	50·0	24·5
„ 29..	11.0	48·6	25·5
„ 30............	11.20	45·8	23·5
May 4............	10.0	48·6	40·0
„ 4............	11.0	56·8	86·0
„ 5............	11.0	51·8	118·0
„ 6............	7.45	51·0	124·0
„ 6............	9.20	52·5	124·5
„ 8............	9.15	59·0	124·0
„ 10............	8.50 A.M.	54·7	143·5

IX. *Note on Beams fixed at the Ends.*

By W. E. AYRTON, *F.R.S., and* JOHN PERRY, *F.R.S.**

I. A HORIZONTAL beam with vertical loads, fixed at the ends, being given, to find the bending-moment everywhere: this is a problem the solution of which has not hitherto been put in an elementary form. A knowledge of the bending-moment everywhere leads, of course, to a complete knowledge of the strength and stiffness of the beam. The problem is quite soluble by a method which is obvious to any one who has worked out the theorem of three moments; but even for advanced students the work is tedious; and practically the answers known for the two cases—(1) when a uniform beam is loaded merely in the middle, (2) when a uniform beam is loaded uniformly—are regarded as roughly applicable in all cases which occur in practice.

If M is the bending-moment at a section, I the moment of inertia of the section about its neutral line, and E Young's modulus of elasticity for the material, then $\frac{M}{EI}$ is the curvature of the beam. If OO′ is a short length of the beam, $\frac{M}{EI}$ OO′

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* Read June 11, 1887.

is the angle which the originally parallel sections at O and O′ now make with one another. Hence, if we divide the beam into a great number of parts, and if M is taken in the middle of each division, the moment of inertia there being I, the sum of all such terms as $\frac{M}{EI} \cdot OO'$ (if OO′ is one of the elementary lengths) gives the angle between the two end sections of the beam.

This principle, that $\Sigma \frac{M}{EI} \cdot OO'$ gives the angle between the two end sections of the beam, is, of course, well known. It is the basis of the theory of flat spiral springs. As applicable to beams, it may be put in the words of Professor Cotterill ('Applied Mechanics,' § 169, p. 334) :—" . . . The angle (*i*) between two tangents to the deflection-curve of a beam is proportional to the area of the curve of bending moments [had he been speaking of non-uniform beams, Mr. Cotterill would have said the area of the curve whose ordinate is $\frac{M}{EI}$] intercepted between two ordinates at the points considered." It is also the basis of Prof. Fuller's beautifully simple method of dealing with arch-ribs fixed at the ends; and, indeed, it was when putting Prof. Fuller's method before our students that we discovered the following simple application of the principle to beams. In spite of the apparent rashness of such a statement about such a well-ventilated subject as the elasticity of beams, we believe our method to be quite new.

II. In the case of a horizontal beam fixed at the ends, the angle between the end sections remains equal to nothing; and hence, in this case,

$$\Sigma \frac{M}{I} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Now, if the beam were merely supported at the ends, from a given system of loading it is easy to find what the bending-moment everywhere would be, either by numerical calculation or a link-polygon method. Let us suppose the diagram of this bending-moment *m* to be known. Then the bending-moment M of a beam fixed at the ends with the same loading, is *m*—*c*, where *c* is a constant; so that it is only necessary from the condition (1) to determine this constant *c*.

(1) is

$$\Sigma \frac{m-c}{\text{I}} = 0, \quad \text{or} \quad \Sigma \frac{m}{\text{I}} = \Sigma \frac{c}{\text{I}},$$

$$\text{or} \quad c = \frac{\Sigma \frac{m}{\text{I}}}{\Sigma \frac{1}{\text{I}}}. \quad . \quad . \quad . \quad . \quad (2)$$

The rule then is, knowing m and I at every point:—Divide the beam into any number, n, of equal parts; find $\frac{m}{\text{I}}$ at the middle of each part, and add all the values together: this is the numerator in (2). Find $\frac{1}{\text{I}}$ at the middle of each part, and add the values together: this is the denominator in (2); so that c is known. Subtract c from every value of m, or diminish all the ordinates of the m diagram by the amount c, and we have the M diagram; that is, the real diagram of bending-moment of the beam fixed at the ends, with any distribution of loading and any variation in cross section.

III. If the cross section is the same everywhere, it is obvious that (2) becomes

$$c = \frac{1}{n} \Sigma\, m, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

if the beam was divided into n parts. In fact, c is the average value of m for the whole span. The application of (3) to beams of uniform section, fixed at the ends with one load in the middle or with a uniformly distributed load, is very simple.

IV. The condition (1) is applicable when I at every cross section of the beam is previously settled in any arbitrary manner. Let us give to I such a value that the beam shall be of uniform strength everywhere, that is that

$$\frac{\text{M}}{\text{I}} y = f_c \text{ or } f_t, \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where y is the greatest distance of any point in the section from the neutral line on the compression- or tension-side, and f_c and f_t are the constant maximum stresses in compression or tension to which the material is subjected in every section.

f_c may generally be taken as equal to f_t; and if d be the depth of the beam, y may generally be taken as equal to $\frac{1}{2}d$. Hence (4) becomes

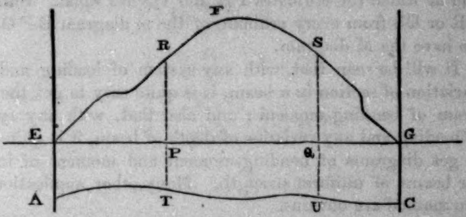
$$\frac{M}{I}d = \pm 2f, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

the + sign being taken when M is positive, and the − sign when M is negative.

Hence (1) becomes $$\Sigma \frac{1}{\pm d} = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

the negative sign being taken from the ends of the beam to the points of inflexion, and the positive sign being taken between the two points of inflexion.

Now to satisfy (6) we have merely to solve the following problem :—In the figure E A C G E is a diagram whose ordinates represent the value of $\frac{1}{d}$, or the reciprocal of the depth of the beam, which may be arbitrarily fixed; E F G E is a diagram of the values of m, easily drawn when the loading is known. We are required to find two points, P and Q, such that



$$\text{area ET} + \text{area QC} = \text{area PU}$$

and

$$PR = QS.$$

When found, these points P and Q are the *points of inflexion*, and PR or QS is what we called c. That is, $m - PR$ is the real bending-moment M at every place; and, knowing the bending-moment and depth d, it is easy to find I, as the condition of uniform strength is given in (5), or

$$I = \pm \frac{M d}{2f},$$

I being of course always positive.

We have found it very easy to solve this problem by trial. First find the area EC. Choose two points P′, Q′, whose ordinates P′R′ and Q′S′ are equal, and such that the area P′U′ seems to the eye nearly half the area EC. Measure the area P′U′, subtract from ½EC, divide by about 4Q′U′ or 4P′T′ or by the mean value of these two. This will give an approximation to the error in the positions of P′ and Q′. With an easy exercise of one's judgment, it will probably not be necessary to make a second approximation.

V. If the depth of the beam is constant, the problem of finding the diagram of bending-moment for a beam of uniform strength is of course solved by finding two points, P and Q, whose distance asunder is half the length EG of the beam, and at which the ordinates PR and QS are equal. Subtract PR or QS from every ordinate of the *m* diagram E F G, and we have the M diagram.

It will be seen that, with any system of loading and any variation of section in a beam, it is quite easy to get the diagram of bending-moment; and also that, with any system of loading and any variation of depth of beam, it is quite easy to get diagrams of bending-moment and moment of inertia for beams of uniform strength. Many other applications of the method are obvious.

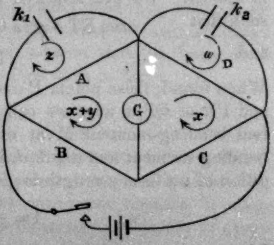
X. *Note on Comparing Capacities.*

By E. C. Rimington*.

Let two condensers, of capacities K_1 and K_2, be connected to the arms A and D of a Wheatstone-bridge, as shown in woodcut; and let

$$AC = BD,$$

so that there is a balance for permanent currents.



Let $x+y$, x, z, and w be the cyclic currents at some moment after the battery-circuit is broken, and when the condensers are discharging; and let q_1 be the charge on K_1 and q_2 that on K_2 at this moment. Then we have

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* Read June 25, 1887.

$$(A+B+G)(x+y)-Gx-Az=0^*,$$
$$(C+D+G)x-G(x+y)-Dw=0;$$

also

$$z=\frac{dq_1}{dt} \quad \text{and} \quad w=\frac{dq_2}{dt}.$$

Therefore

$$(A+B)x+(A+B+G)y=A\frac{dq_1}{dt},$$

$$(C+D)x-Gy=D\frac{dq_2}{dt}.$$

Hence

$$y=\frac{\begin{vmatrix} A\frac{dq_1}{dt}, & A+B \\ D\frac{dq_2}{dt}, & C+D \end{vmatrix}}{\begin{vmatrix} A+B+G, & A+B \\ -G, & C+D \end{vmatrix}},$$

or

$$\Delta y = A(C+D)\frac{dq_1}{dt}-(A+B)D\frac{dq_2}{dt}, \quad . \quad . \quad (1)$$

where

$$\Delta = G(A+B)+(A+B+G)(C+D).$$

Integrating,

$$\Delta\int_0^\infty y\,dt = A(C+D)\int_{K_1Ax_0}^0 dq_1-(A+B)D\int_{K_2Dx_0}^0 dq_2,$$

where x_0 is the permanent current through A and D when the battery is on.

But $\int_0^\infty y\,dt$ is the quantity which passes through the galvanometer, $=q$.

Therefore

$$\Delta q = x_0\{K_2D^2(A+B)-K_1A^2(C+D)\}.$$

If x_1 is the permanent current in B and C,

$$x_0=x_1\frac{C}{D};$$

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* The self-induction of the galvanometer need not be taken into account, as the current in it commences and ends at zero. The arms A, B, C, and D are supposed to possess no self-induction.

and since AC=BD, we have

$$q = x_1 \mathrm{BD} \frac{\mathrm{K_2C - K_1B}}{\mathrm{G(B+C)+B(C+D)}}, \quad . \quad . \quad . \quad . \quad (2)$$

which may be written

$$q = x_1 \frac{\mathrm{K_2C - K_1B}}{\frac{\mathrm{G(B+C)}}{\mathrm{BD}} + \frac{\mathrm{C}}{\mathrm{D}} + 1}.$$

If now A and D be made infinite, which will obviate the necessity of first adjusting for a permanent balance,

$$q = x_1 (\mathrm{K_2C - K_1B}).$$

Also

$$x_1 = \frac{\mathrm{E}}{\rho + \mathrm{B} + \mathrm{C}},$$

where ρ is the internal resistance and E the E.M.F. of the testing battery.

$$q = \frac{\mathrm{E}}{\rho + \mathrm{B} + \mathrm{C}} (\mathrm{K_2C - K_1B}).$$

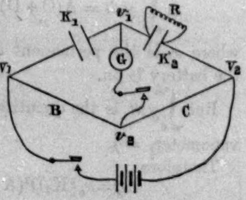
If $q=0$,

$$\frac{\mathrm{K_1}}{\mathrm{K_2}} = \frac{\mathrm{C}}{\mathrm{B}}.$$

If a key be also put in the galvanometer-circuit, and the battery-key be first depressed and then, after a certain interval of time, the galvanometer-key, we have Gott's method of comparing capacities. This allows the condenser longer time to charge; and if the galvanometer when its key is depressed shows no throw, it indicates that the potentials v_1 and v_2 are equal, and that therefore

$$\frac{\mathrm{K_1}}{\mathrm{K_2}} = \frac{\mathrm{C}}{\mathrm{B}},$$

as before.



If, however, either of the condensers has an appreciable leakage, the result will be false by this method.

Suppose the condenser $\mathrm{K_2}$ has an insulation-resistance R, that of $\mathrm{K_1}$ being infinite. On depressing the battery-key, as the condensers are in series, they take initial charges each equal to Q.

$$Q=(V_1-v_1)K_1=(v_1-V_2)K_2=(V_1-V_2)\frac{K_1K_2}{K_1+K_2}.$$

Let an interval of time t elapse before the galvanometer-key is depressed, and at the end of it let Q_1 and Q_2 be the charges on K_1 and K_2 respectively, and v the value of v_1. Then

$$Q_2=Qe^{-\frac{t}{K_2R}},\text{ where } e=2{\cdot}718,$$

$$Q_2=(V_1-V_2)\frac{K_1K_2}{K_1+K_2}e^{-\frac{t}{K_2R}}$$

and

$$Q_1=(V_1-v)K_1=\{(V_1-V_2)-(v-V_2)K_1$$

$$=\left\{(V_1-V_2)-\frac{Q_2}{K_2}\right\}K_1,$$

$$Q_2=(V_1-V_2)\left(1-\frac{K_1}{K_1+K_2}e^{-\frac{t}{K_2R}}\right)K_1.$$

Now

$$(v-V_2)=\frac{Q_2}{K_2}=(V_1-V_2)\frac{K_1}{K_1+K_2}e^{-\frac{t}{K_2R}}$$

and

$$(V_1-v)=\frac{Q_1}{K_1}=(V_1-V_2)\left(1-\frac{K_1}{K_1+K_2}e^{-\frac{t}{K_2R}}\right).$$

Hence

$$\frac{v-V_2}{V_1-v}=\frac{K_1e^{-\frac{t}{K_2R}}}{K_1(1-e^{-\frac{t}{K_2R}})+K_2}.$$

Also

$$\frac{v_2-V_2}{V_1-v_2}=\frac{C}{B}.$$

If, therefore, $v=v_2$, or there appears to be a balance,

$$\frac{C}{B}=\frac{K_1e^{-\frac{t}{K_2R}}}{K_1(1-e^{-\frac{t}{K_2R}})+K_2}$$

or

$$K_1=\frac{K_2C}{(B+C)e^{-\frac{t}{K_2R}}-C}.$$

So that, unless t be made very small compared to K_2R, we cannot compare K_1 and K_2 unless we know R and t. In the case of no leakage the conditions will be the same as if there were no key in the galvanometer circuit.

To find the best conditions for the test:—Taking equation (2), and substituting for x_1 its value

$$\frac{E}{\rho+\frac{(A+D)(B+C)}{A+D+B+C}}\times\frac{D}{C+D},$$

which equals $\dfrac{ED}{\rho(C+D)+D(B+C)}$, since $AC=BD$, we obtain

$$q=\frac{EBD^2(K_2C-K_1B)}{\{\rho(C+D)+D(B+C)\}\{G(B+C)+B(C+D)\}}.$$

If $q=0$, $K_2C=K_1B$.

Suppose a fractional error p to be made in the value of C. Then

$$q=\frac{EBD^2pK_2C}{\{\rho(C+D)+D(B+C)\}\{G(B+C)+B(C+D)\}}$$

$$=\frac{EpK_2}{\left(\frac{\rho}{D}+\frac{\rho}{C}+\frac{B+C}{C}\right)\left(\frac{G}{D}+\frac{GC}{BD}+\frac{C+D}{D}\right)}.$$

Now the throw is proportional to $q\sqrt{G}$ for a galvanometer whose coil-volume is constant. Therefore the throw is proportional to

$$\frac{EpK_2}{\left(\frac{\rho}{D}+\frac{\rho}{C}+\frac{B+C}{C}\right)\left(\frac{\sqrt{G}}{D}+\frac{C\sqrt{G}}{BD}+\frac{C+D}{D\sqrt{G}}\right)},$$

and this is to be a maximum; which it obviously is when

$$\sqrt{G}\,.\,\frac{B+C}{BD}=\frac{C+D}{D}\frac{1}{\sqrt{G}},\quad\text{or}\quad G=\frac{B(C+D)}{B+C}.$$

If A and D are infinite, the higher the resistance of the galvanometer the better. Also, in this case,

$$q=\frac{EpK_2}{\frac{\rho}{C}+\frac{B}{C}+1}=\frac{EpK_2}{\frac{\rho}{C}+\frac{K_2}{K_1}+1}.$$

Here q is largest when C is largest.

To find the conditions under which a telephone may replace the galvanometer :—

In equation (1) we have

$$\Delta y = A(C+D)\frac{dq_1}{dt} - (A+B)D\frac{dq_2}{dt}.$$

Now, in order to employ a telephone, y must always be zero. Therefore

$$\frac{A}{A+B}\frac{dq_1}{dt} = \frac{D}{C+D}\frac{dq_2}{dt}.$$

But

$$\frac{A}{A+B} = \frac{D}{C+D}, \text{ since } AC = BD.$$

Hence

$$\frac{dq_1}{dt} = \frac{dq_2}{dt}.$$

Now, since there is no current in the galvanometer, its resistance may be anything: let it be zero. Then K_1 discharges through $\frac{AB}{A+B}$, and K_2 through $\frac{CD}{A+D}$.

Consequently

$$q_1 = x_0 K_1 A e^{-\frac{t}{K_1\frac{AB}{A+B}}},$$

and

$$q_2 = x_0 K_2 D e^{-\frac{t}{K_2\frac{CD}{C+D}}}.$$

Hence

$$\frac{dq_1}{dt} = -x_0 K_1 A \,.\, \frac{A+B}{K_1 AB} e^{-\frac{t}{K_1\frac{AB}{A+B}}}$$

$$= -x_0 \frac{A+B}{B} e^{-\frac{t}{K_1\frac{AB}{A+B}}},$$

and

$$\frac{dq_2}{dt} = -x_0 \frac{C+D}{C} e^{-\frac{t}{K_2\frac{CD}{C+D}}}.$$

Therefore

$$\frac{A+B}{B} e^{-\frac{t}{K_1\frac{AB}{A+B}}} = \frac{C+D}{C} e^{-\frac{t}{K_2\frac{CD}{C+D}}},$$

and

$$\frac{A+B}{B} = \frac{C+D}{D}.$$

Therefore

$$K_1\frac{AB}{A+B}=K_2\frac{CD}{C+D}.$$

and, since $AC=BD$,

$$K_1B=K_2C,$$

$$\frac{K_1}{K_2}=\frac{C}{B}.$$

If there is self-induction in the four conductors, let L_1 be the coefficient of self-induction of A, L_2 of D, L_3 of B, and L_4 of C. Then

$$(A+B+G)y+(A+B)x+L_1\frac{d(x+y)}{dt}+L_3\frac{d(x+y)}{dt}-A\frac{dq_1}{dt}=0.$$

$$-Gy+(C+D)x+L_2\frac{dx}{dt}+L_4\frac{dx}{dt}-D\frac{dq_2}{dt}=0.$$

Solving for y, we have

$$\Delta y=(C+D)\left\{A\frac{dq_1}{dt}-L_1\frac{d(x+y)}{dt}-L_3\frac{d(x+y)}{dt}\right\}$$
$$-(A+B)\left\{D\frac{dq_2}{dt}-L_2\frac{dx}{dt}-L_4\frac{dx}{dt}\right\}.$$

Integrating,

$$\Delta q=(C+D)\left\{A\int_{K_1Ax_0}^{0}dq_1-L_1\int_{x_0}^{0}d(x+y)-L_3\int_{-\frac{A}{B}x_0}^{0}d(x+y)\right\}$$
$$-(A+B)\left\{D\int_{K_2Dx_0}^{0}dq_2-L_2\int_{x_0}^{0}dx-L_4\int_{-\frac{A}{B}x_0}^{0}dx\right\}$$
$$=x_0\frac{C+D}{C^2}(BCD^2K_2-B^2D^2K_1+L_1C^2-L_3CD-L_2BC+L_4BD),$$

since $AC=BD$.

Also

$$x_0=x_1\frac{C}{D},$$

where x_1 is the permanent current in the branches B and C.

$$\therefore\ \Delta q=x_1.\frac{C+D}{CD}(BCD^2K_2-B^2D^2K_1+L_1C^2-L_3CD$$
$$-L_2BC+L_4BD).$$

If $q=0$,

$$BD^2(K_2C-K_1B)+C(L_1C-L_3D)+B(L_4D-L_2C)=0.$$

Hence

$$K_2C=K_1B,\quad L_1C=L_3D,\quad L_4D=L_2C;$$

or

$$\frac{K_1}{K_2}=\frac{C}{B}\quad\text{and}\quad\frac{L_1}{L_3}=\frac{L_2}{L_4}=\frac{D}{C}.$$

Again, since

$$\Delta = \frac{C+D}{C}\{B(C+D)+G(B+C)\},$$

$$q = x_1 \frac{BCDK_2 - B^2DK_1 + L_1\frac{C^2}{D} - L_2\frac{BC}{D} - L_3C + L_4B}{B(C+D)+G(B+C)}$$

$$= x_1 \frac{K_2BC - K_1B^2 + L_1\frac{C^2}{D^2} - L_2\frac{BC}{D^2} - L_3\frac{C}{D} + L_4\frac{B}{D}}{\frac{BC}{D} + B + \frac{G(B+C)}{D}}.$$

If A and D are now made infinite,

$$q = x_1(K_2C - K_1B);$$

the same result that we obtain if there is no self-induction in the branches B and C.

XI. *The Effect of Change of Temperature in Twisting or Untwisting Wires which have suffered Permanent Torsion. By* HERBERT TOMLINSON, *B.A.*[*]

IN a paper recently communicated to the Physical Society[†], Mr. R. H. M. Bosanquet has drawn attention to the remarkable behaviour of a very fine hard-drawn platinum wire, which twisted or untwisted very appreciably when subjected to small variations of temperature. As some experiments made by myself nearly eight years ago, but of which the results have not been as yet published, seem to bear on the interesting phenomenon in question, I now venture to bring them forward. I had previously been investigating the effects of permanent extension and compression on the longitudinal elasticity of wires in the following manner:—A wire of from 60 to 90 centimetres in length and 1 millimetre in diameter was suspended vertically, with its upper extremity clamped to a rigid support, and its lower one attached to a little brass block provided with a hook at the bottom, to which weights could be attached. Permanent torsion was imparted to the wire, which was afterwards subjected to various amounts of longitudinal stress. The permanent twisting of the wire in the direction of a right-handed screw causes the portion A B C D (fig. 1) to be permanently extended along the diagonal A C and compressed along the diagonal B D (fig. 2).

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[*] Read June 25, 1887.

[†] *Ante*, p. 50.

Fig. 1.

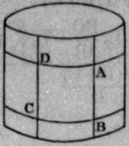
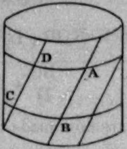


Fig. 2.



If now the wire be loaded, the stress will produce temporary twist or untwist according as the elasticity is greater or less in the direction B D than in the direction A C. A full account of these experiments will be found in the 'Philosophical Transactions,'[*] so that it will suffice to state here that all the wires examined showed temporary twist or untwist to be the result of loading. The amount of twist or untwist produced by loading was comparatively small, and was measured by the usual mirror lamp-and-scale arrangement, the mirror being attached to the brass block at the end of the wire.

It then occurred to me that a similar arrangement might be used for testing the effect of permanent extension and compression on the thermal expansibility of a metal. A wire was suspended in the axis of an air-chamber, consisting of two concentric brass cylinders enclosing an annular space between them 6 millim. thick. The length of the air-chamber was 120 centim. and its outer diameter 10 centim. When the required amount of permanent torsion had been imparted to the wire, steam from a boiler was admitted into the annular space through a tube soldered into it near its lower extremity, and passed out through a similar tube near the top, where it was condensed in a vessel of cold water. After a period ranging from ½ hour to 2 hours, when the *permanent* untwisting which invariably accompanied the heating had ceased to become sensibly greater, the position of the light on the scale was noted and the action of the steam stopped. The annular space was now filled with cold water, and in about ten minutes afterwards the position of the light on the scale was again noted. The difference between the two readings was a measure of the amount of temporary twist or untwist produced by the change of temperature, the last being, on the average, 85° C. In several cases the heating and cooling were repeated with little or no variation in the amount of temporary twist or untwist. The length of each wire was nearly 120 centimetres and the diameter 1 millimetre.

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[*] Part I. 1883: "The Influence of Stress and Strain on the Physical Properties of Matter."

The results of the experiments will be found in the following Table, in which are given the amount of temporary twist or untwist in scale-divisions, the amount of permanent untwist, and the load hanging on the wire during the experiment. The wires were all well annealed before being permanently twisted.

Metal ...	Copper.		Iron.		Aluminium		Silver.		Platinum.	
Load on the wire, in kilos.	1		1		½		1		1	
Number of complete turns of permanent torsion.	Permanent untwist produced by heating. P.	Temporary effect of heating. + signifies temporary twist, T.	P.	T.	P.	T.	P.	T.	P.	T.
10	......	.. ...	24	3·5−	800	74−	298	2·5−	...	4+
20	......	31+	...	......	...	71−				
30	......	......	...	6·0−						
40	......	......	...	......	...	......	...	3·0−		
50	555	36+	...	......	...	74−	...	......	63	10+
100	690	41+	...	5·0−	...	......	...	3·0−	138	
150	......	......	...	......	...	78−	...	......	273	12·5+
200	861	......	58	......	...	......	...	......	...	13·0+

We may gather from the Table the following particulars:—

(1) Rise of temperature from 15° C. to 100° C. produces in permanently twisted iron, aluminium, and silver temporary

untwist, showing for these metals less thermal expansibility in the direction of permanent extension than in the direction of permanent compression. With copper and platinum, on the contrary, rise of temperature produces temporary twist.

(2) The amount of temporary twist or untwist is in all cases small compared with the amount of permanent untwist produced by rise of temperature. It is also absolutely very small*.

(3) A few turns of permanent torsion suffice to produce the maximum alteration of thermal expansibility which can be effected by this means.

Small variations of the load hanging on the wire produced no sensible effect on the temporary twist or untwist following on rise of temperature. The case, however, was different when large loads were employed. With platinum, for example, by increasing the load from 1 to 7 kilos. the temporary twist produced by rise of temperature was increased threefold. With copper also there was a similar effect produced by largely increasing the load. On the contrary, it would seem that with these metals, in which untwist is produced by rise of temperature, increase of load diminishes the untwist; and if the load be sufficiently large, the wire begins to be twisted temporarily by rise of temperature. This was found to be the case with annealed iron. The wire was twisted permanently with gradually increasing amounts of load on it, and after each twisting was slightly heated by running a burner up and down it. As before, with moderate loads there was temporary untwist on heating; when the load reached 10 kilos. there was neither temporary twist nor untwist; and finally, when the load was 16 kilos., there was very appreciable temporary twist.

Annealed piano-steel behaved like annealed iron; but with unannealed piano-steel rise of temperature caused a very appreciable temporary *twist*, instead of untwist, even with small loads on.

A remarkable feature about all these experiments was the great difference with different metals in the facility with which the effect of permanent torsion in one direction could be reversed by permanent torsion in the opposite direction. With copper, for example, one complete revolution in the opposite direction was sufficient to reverse the effect of 200 complete turns of permanent torsion, whilst with iron the difficulty of reversing the effect of previous torsion was considerable.

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* 100 scale-divisions only represent torsion through an angle of 2° 20′.

I am inclined to believe that the phenomenon observed by Mr. Bosanquet is to be, at any rate partly, attributed to unequal expansion in different directions. I have always found in wires which have been hard-drawn a certain amount, and sometimes a considerable amount of permanent torsion: this, we have seen, will cause temporary twist or untwist to be produced by rise of temperature. The amount of twist or untwist in any case, however, observed by myself was very much less than that observed by Mr. Bosanquet; and it would be of interest to ascertain how far the comparatively very large variation of torsion with small rise of temperature which occurred with the platinum wire used by him, is to be attributed to the comparatively great longitudinal stress on the wire*.

XII. *Remarkable Effect on raising Iron when under Temporary Stress or Permanent Strain to a Bright-red Heat.* By HERBERT TOMLINSON, *B.A.*†

IT has been shown in the preceding paper that an annealed iron wire which has been permanently twisted is temporarily untwisted when the temperature is raised to 100° C., provided there is not too great a load on the end of the wire; but that if the load on the end of the wire is sufficiently great, a temporary twist follows on the rise of temperature. Fresh experiments were therefore entered on with the view of ascertaining whether, with a small load on the end of the wire, the temporary untwist produced by rise of temperature would be changed to twist when the temperature exceeded a certain limit. A few preliminary trials were made by merely heating a portion of the permanently twisted wire with a burner; and it was found that when the wire reached a bright red heat a most remarkable and sudden change occurred, the wire sharply *twisting* in the same direction as that in which it had previously suffered permanent torsion. When the burner was removed and the wire was cooled to a temperature which seemed slightly lower than that at which the phenomenon occurred in the first instance, it as suddenly untwisted; and when it had again attained the temperature of the room, there remained a decided *permanent twist in addition to that which had been originally imparted**. This additional permanent twist is rendered more remarkable by the fact that rise of temperature beyond a dull red, but not extending to a bright red, is attended with a considerable *permanent untwist*. The phenomenon is evidently closely associated with one discovered by Mr. Gore so far back as 1869, and described in the 'Proceedings of the Royal Society' for that year. Mr. Gore's experiments were conducted in the following manner:—A thin iron wire, fixed at one end to a binding-screw, is attached at the other to an index which multiplies any motion of the wire; the wire is stretched horizontally by a feeble spring, and is heated by an electric current or by a row of gas-jets. According to Mr. Gore, no anomalous action is observed on *heating* the wire to bright incandescence; but when cooling begins, the index moves back until a moderate red heat is attained, when suddenly the pointer gives a jerk, indicating a momentary *elongation* of the wire during the progress of its contraction. In 1873 Professor Barrett extended Mr. Gore's researches; and in a paper full of interest †, entitled "Certain remarkable Molecular Changes occurring in Iron Wire at a low red Heat," showed that, under suitable conditions, the sudden change could be observed *not only on cooling, but on heating*. It is apparent, from the description given above, that the phenomenon observed by Mr. Gore resembles the one observed by myself, in that there is a sudden change in the iron when it has been raised to a bright red and afterwards cooled to a certain temperature. But in Mr. Gore's experiment the wire was under temporary *stress*, whilst in mine it was permanently *strained*; and whereas in the one case, on cooling below a certain temperature, there was a sudden *yielding* to the *stress*, in the other there was a sudden *diminution* of permanent *strain*. A careful consideration of the results of my own experiments, and of those of Mr. Gore and Professor Barrett, satisfied me that in this, as in many other

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* See above, the effect of increasing the load on the wire.

† Read June 25, 1887.

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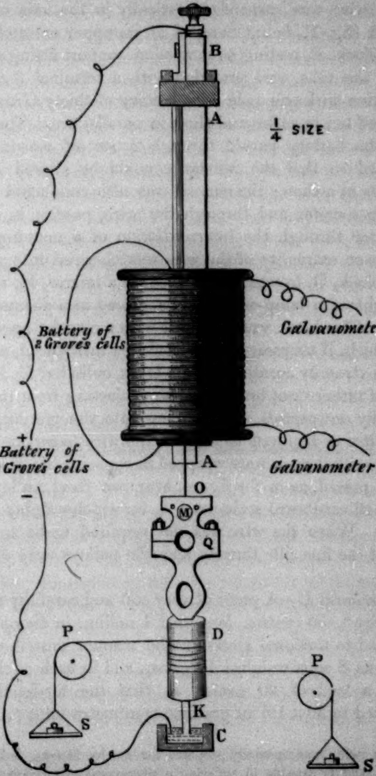
* This is not so if the wire be heated rather slowly; but, on the contrary, in this case there is a very decided *permanent untwist* (see experiment V.).

† Phil. Mag. ser. 4, vol. xlvi. p. 472.

Fig. 1.


B
A
$\frac{1}{4}$ SIZE
Battery of 2 Grove's cells
Galvanometer
+
Battery of 30 Grove's cells
−
A
Galvanometer
O
M
Q
D
P
P
K
S
C
S

instances, temporary stress and permanent strain act in opposite directions as regards their effects on the physical properties of matter. I was moreover led to test, not only the effects of torsional stress and strain, but also those of other mechanical stresses and strains, for the most part with arrangements which will now be described.

The wire was suspended vertically in the axis of a glass tube, A (fig. 1), being clamped at its upper extremity into a brass block, B, resting on a wooden support fitting on to the top of the tube, and provided with a terminal for making connexion with one pole of a battery of thirty Grove's cells, arranged ten in series and three in parallel arc. The current from the battery passed through a set of resistance-coils arranged so that the resistance could be altered by small amounts at a time; the current was also conducted through an amperemeter and through the wire, passing in or out of the latter through the intermediation of a mercury-cup, C. The lower extremity of the wire was clamped into a second brass block, O, to which was secured a mirror, M, reflecting the light of a lamp on to a scale placed at a distance of one metre. This block was provided with a circular aperture, Q, into which, if necessary, a bar could be introduced, and at its lower extremity terminated in a brass cylinder, D, having a piece of rather stout brass wire, K, projecting from its centre, vertically downwards, and dipping into the mercury-cup C. When it was required to subject the wire to torsional stress, two fine silk threads were wrapped in opposite directions round D, and passed, as in the figure, over two fixed pulleys, P, to two small cardboard scale-pans, S, on which weights could be placed. When the wire was not required to be torsionally stressed the fine silk threads and the pulleys were dispensed with.

Experiment I.—A piece of very soft and carefully annealed iron wire*, 30 centim. long and 1 millim. in diameter, was subjected to torsional stress in the manner described above. The pans S each weighed 10 grms., and in each of them was placed a load of 20 grms., so that the torsional couple amounted to 30 × 1·6 in gramme-centimetre-units†. A current of gradually increased amount was passed through the wire, and when the temperature approached a bright red the wire began to twist rapidly and permanently under the influence of the stress*. The current was shortly afterwards stopped; and when a temperature between bright red and dull red had been reached the wire began suddenly to twist further, the amount of the sudden twist being about 90 degrees. When the wire had cooled down to the temperature of the room there was left a considerable permanent twist. In this experiment there was no perceptible temporary untwist on heating, but merely a slight check in the rate at which the wire was permanently twisting as soon as the critical temperature was reached. Other experiments were made with smaller and smaller torsional stresses until only the cardboard scale-pans were left to produce torsion: even here, however, it was impossible to get any sign of temporary untwist on heating. This mode of producing torsion was accordingly abandoned, and the stress was applied in the manner described in Experiment II.

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* This wire was specially prepared for me by Messrs. Johnson and Nephew, and is capable of suffering a permanent elongation of 25 per cent. before breaking.

† The diameter of D was 1·6 centimetre.

Experiment II.—A cork was fitted into the central aperture of the block O, and through the centre of the cork was passed a knitting-needle about 25 centim. in length and 1½ millim. in diameter, which was magnetized rather feebly by rubbing it with a small bar-magnet. The knitting-needle, when first placed in position, was nearly in the magnetic meridian, but afterwards the block B was turned until the needle eventually was nearly at right angles to the plane of the magnetic meridian, so that the wire might be under a feeble torsional stress due to the action of the earth's horizontal magnetic force on the needle. Immediately the wire reached a bright red temperature there was a very perceptible sudden temporary untwist, and on cooling a sudden temporary twist at nearly the same temperature †. This experiment was repeated with another piece of the same iron wire; but now the needle, instead of being twisted through 90 degrees from the north and south position, in which it originally lay when there was no torsion on the wire, was only twisted through about 10 degrees. Even the extremely feeble torsional stress now acting on the wire was not only sufficient to produce the phenomenon, but also a very decided *permanent* twist. The permanent twisting began directly the wire, on being heated, reached *a dull red heat*, and continued with increasing rapidity until the temporary untwist occurred at a bright red heat.

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* The *first* appearance, however, of sensible permanent twist occurred rather suddenly at *a dull red heat.*

† These effects can easily be shown by merely heating a small portion of the wire with a Bunsen-burner.

Experiment III.—The temperature at which, *on cooling*, the sudden change took place was evidently very much higher than that at which it occurred with the specimens of iron used by Mr. Gore and Professor Barrett; and this I felt inclined to attribute to the comparative softness of the iron used by myself. I accordingly tried several other specimens of iron and steel, both in the annealed and in the unannealed condition. With some of these the phenomenon did not occur on *cooling* until a dull red, or even at a still lower temperature; but it was found essential to its production that the iron *should have been previously raised to the temperature of bright incandescence**. On *heating*, the untwisting always showed itself at the temperature of bright red.

In consequence of the sudden change on cooling not occurring till a dull red heat had been reached in his experiments, Professor Barrett connected the phenomenon in question with another of equal interest. It is well known that, at a temperature of dull red, iron begins very rapidly to lose its magnetic properties; and, according to Professor Barrett, the two phenomena occur simultaneously. It is no doubt the case that, on *cooling*, the sudden jerk occurs simultaneously with the sudden regaining of magnetic properties in *some* specimens† of iron or steel; but it by no means follows that we have not two very distinct critical temperatures—one at or about a dull red, at which iron loses or regains its magnetic properties according as the wire is being heated or cooled; and another at a much higher temperature, namely near a bright red, at which sudden changes, certainly not less profound, occur. This is shown in the next experiment, which at the same time illustrates the fact already mentioned, that the effect of permanent strain is opposite to that of temporary stress.

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* This fact, which was also noticed by Mr. Gore and Prof. Barrett, is significant.

† The reason of this will be found in the remarks on the recalescence of iron.

Experiment IV.—The glass tube (fig. 1) was placed inside a magnetizing solenoid*, consisting of a single layer of cotton-covered copper wire $\frac{1}{20}$ inch diameter, and connected through a key with a battery of two Grove's cells. Surrounding the solenoid and concentric with it is a secondary coil, consisting of 840 turns of cotton-covered copper wire partly $\frac{1}{20}$ inch diameter and partly $\frac{1}{10}$ inch diameter. This coil has a resistance of about 1 ohm, and is connected through a key with a very delicate Thomson's reflecting-galvanometer of about 7 ohms' resistance. A piece of the same soft iron wire already mentioned was subjected to thirty complete revolutions of permanent torsion, and was finally released from all torsional stress. When the wire, on heating, reached the temperature of dull red, a momentary deflection of the needle of the Thomson's galvanometer took place, indicating a sudden loss of magnetic permeability; and as soon as a bright red heat had been attained it, as in the preliminary observations, *twisted* sharply and suddenly. The battery which was employed to heat the wire had its circuit now broken, and the wire was allowed to cool: first ensued a sudden momentary *untwist* of the wire, whilst *a few seconds afterwards* a kick of the galvanometer-needle in the opposite direction to that which had occurred on heating, indicated that the iron had suddenly regained its magnetic properties. This experiment was repeated several times; and though the sudden jerk on heating and cooling became less and less in intensity, and probably would eventually have vanished, it did not do so after six heatings and coolings†.

Experiment V.—The last experiment had shown the great persistency of the permanent torsional strain, even after the wire had been several times heated to a white heat. A fresh series of trials was made with iron which had suffered more or less permanent torsion, for the purpose of examining this persistency more closely. In these trials it was attempted to reanneal the wire after the permanent torsion had been imparted, by passing the flame of a Bunsen-burner very slowly up and down it so as to heat all parts in turn to a white heat, the wire being in some cases entirely free from any load on it, and in others having merely the slight load due to the block and its appendages. In no case could the sudden jerk at a bright red heat be got rid of entirely; though the same wire, when tested previously to imparting permanent torsion, had shown no trace whatever of the phenomenon. It is evident that iron cannot be satisfactorily annealed by the process mentioned above*.

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* Only the ends of this solenoid can be seen in the figure, the rest being enveloped by the secondary coil.

† I had the pleasure of repeating the experiment before the Physical Society.

These trials also brought out distinctly the fact that at a dull red heat the iron begins to *permanently* untwist rapidly, so that a considerable amount of the permanent twist originally imparted can be got rid of by maintaining the temperature for some time between bright red and dull red. Directly, however, the higher critical temperature is reached there is a sudden twist, which in the case of very soft iron is partly temporary and partly permanent, and in the case of steel wire which is unannealed, or hard iron wire, is principally permanent†.

Experiment VI.—A piece of the well-annealed iron wire, about 16 inches long, was clamped at one end and sustained in a horizontal position, save in so far as it was bent by its own weight. The wire was heated by a burner about three inches from the clamp. When the temperature reached a bright red, the wire began to bend permanently very rapidly by its own weight; and when the burner was removed and the wire cooled, a *sudden further bending took place* at a temperature a little below bright red; and when cooled to the temperature of the room, the wire remained permanently bent. Here the attempt to unbend, which the wire no doubt made at the critical temperature when it was *heated*, was masked by the permanent bending. Another piece was therefore taken and heated at a point further away from the clamp, so that the bending-stress should not be so great. With care it was found possible to detect that the wire suddenly *straightened* itself when the critical temperature was reached on *heating*.

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* I write this because some observers seem to think that iron can be satisfactorily annealed in this way. In this, I believe, they are mistaken the iron should be maintained at a high temperature for a considerable period, and afterwards be allowed to cool very slowly.

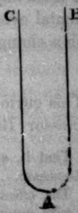
† With some unannealed specimens I found it difficult to detect any trace whatever of *untwisting* temporarily when the iron *cooled* below the critical temperature.

Similar trials were made with other specimens of iron and steel not so well annealed; and with these, when cooling, the sudden bending took place at a temperature lower than that at which it occurred with the very soft iron. With one specimen of wire, 1½ millim. thick, it occurred at a temperature *apparently* below that of visible red*; but, as Professor Barrett justly remarks, the internal temperature of the thicker wires is no doubt masked by the cooling of the surface, whereas in thin wires the cooling throughout is extremely rapid. I found also that a piece of this specimen of iron *appeared* to lose its magnetic properties at a temperature below visible red, whereas with the thinner wires the *apparent* temperature at which this took place was somewhere about a dull red.

The very small amount of bending-*stress* which is required to bring out the phenomenon makes it a little difficult to detect with certainty the *opposite* effect of bending-*strain*; but it may be managed after a few trials in the following manner:—

Experiment VII.—A piece of the very soft iron wire was bent, as in fig. 2, with the portions A C, A B in a vertical position; the end C was secured to a clamp, and the bend A heated by a burner. If A B has been so arranged that its centre of mass is very nearly vertically above the part heated, there will be little bending-stress. In this experiment, on heating the bend A to a bright red, the end B jerked suddenly towards C, and when the burner was removed almost immediately jerked back again. If A B is arranged so that there is a little more bending-stress, it is curious to notice the struggle which sometimes ensues at a bright red heat as to

Fig. 2.



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* Prof. Barrett also remarks that the phenomenon occurs on cooling at an *apparently* lower temperature with thick wires than thin ones.

which way the end B shall be jerked*. This experiment was repeated with other specimens of iron and steel and with similar results, except that on cooling, as with the torsional stress and strain, the phenomenon occurred at a lower temperature than with the very soft iron wire.

The question now arises, Is there any sudden *molecular* change at a bright red heat even where there is *no stress or strain affecting the wire?* Professor Barrett brings forward evidence in support of the view that there is, which I am afraid is not conclusive. He says:—"If, however, this molecular change be entirely due to alteration in cohesion, then the removal of the spring ought to cause the anomalous behaviour to disappear. But it does not. Without the spring, an iron wire can be seen by the naked eye to undergo a momentary contraction during heating, and a momentary and more palpable elongation during cooling†. Fixing one end of the wire, and bending the other extremity at right angles so that it may dip into a trough of mercury, and thus preserve contact with the battery, both actions can be seen." I have little doubt myself that the effects observed here were really due to the stress produced by the *weight of the wire itself*. So far as my own experiments go, I have not been able to detect any sudden change in the wire when sufficient care is taken to prevent the weight of the wire itself from producing an effect‡. Nevertheless I am inclined to believe, with Professor Barrett, that a sudden *molecular* change does occur at the critical temperature even when the wire is quite free from any mechanical stress or strain, though as yet experimental evidence is wanting to furnish sufficient evidence of such a change.

The Recalescence of Iron.

This curious phenomenon was, I believe, first discovered by Professor Barrett, and mention is made of it in his paper,

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[*] That is, as to whether the effect of stress or that of strain shall predominate.

[†] The effect on cooling could always be got in my own experiments also with greater distinctness than the effect on heating.—H. T.

[‡] It is difficult to realize, without actually putting the matter to the test of experiment, what a very small amount of stress or strain suffices to bring out the phenomenon.

already alluded to, in the following words :—"On September 12th I was examining the condition of the wire in a darkened room, when a new and unexpected change revealed itself. During the cooling of the wire it was found that just as it reached a very dull red heat, a sudden accession of temperature occurred, so that it glowed once more with a bright red heat. Illuminating the index and scale of the apparatus, which was watched by an assistant, it was at once found that the *reheating of the wire occurred simultaneously with the momentary elongation.*" Professor Barrett also ascertained that, in order to bring out the phenomenon, it was necessary *previously to heat the wire to a white heat*; and, further, that *wherever the momentary expansion of the wire is feeble or absent, there likewise this recalescence is also feeble or absent.*

In my own mind I have little doubt that this beautiful phenomenon may be accounted for in the following manner:—When the iron has been heated above a bright red and is then cooling, it reaches the critical temperature at which there is a *tendency* for the sudden change to take place in the permanently strained or temporarily stressed wire: the change does not, however, *actually* take place at this temperature in consequence of the so-called coercitive force of the iron. When the iron is very soft and well annealed, the temperature at which the jerk takes place is comparatively near the temperature at which the jerk occurred on heating. But when the iron is hard-drawn or only imperfectly annealed, there may be a considerable difference in the two temperatures. Suppose, then, that the iron has cooled to the temperature at which its magnetic properties are suddenly restored. At this temperature a commotion more or less profound takes place among the molecules; and this is sufficient to give them a start towards those positions which they have all along, after the higher critical temperature was passed, been trying to assume. When once started the molecular motion continues, the energy of position is rapidly converted into the energy of motion, and this again into the energy of heat, so that the wire once more glows. It by no means follows that the phenomenon of recalescence occurs with *every* specimen of iron at the critical temperature at which iron loses its magnetic properties; indeed I have noticed sometimes *more than one* evident sudden accession of heat[*] during the *same* cooling. Moreover, in the case of the very soft iron wire, with which the sudden jerk occurred at a much higher temperature on cooling than with the harder specimens, there was no perceptible trace of the phenomenon at a dull red heat. Probably there was a sudden check of the rate of cooling when the jerk occurred, though reglowing at this point was not noticed. I did not, however, pay much attention to the matter.

The view just advanced respecting the phenomenon of recalescence is, I think, justified by the following considerations:—

(1) The phenomenon is entirely absent in very well-annealed iron wire which has not, after annealing, suffered strain, nor is at the time under stress.

(2) It is not sensible unless the temperature of the wire has been *previously* raised to a bright red[†].

(3) It becomes more and more sensible as the temperature at which the sudden jerk occurs in the wire becomes lower and lower, and is most pronounced in those wires in which the jerk does not take place until the wire suddenly loses its magnetic properties.

(4) It is not sensible unless there is a sudden jerk; the jerk and recalescence occur at the same instant.

Gore's phenomenon and the phenomenon of recalescence do not occur in pure, or nearly pure, specimens of nickel or cobalt; though, as is well known, nickel loses its magnetic properties at a much lower temperature than iron, namely from about 350° C. to 400° C. A most careful examination of three different specimens of nickel wire, procured from Messrs. Johnson and Matthey, was made[‡]. Unfortunately pure nickel wire cannot be drawn; but two out of the three specimens showed no trace whatever of the phenomena. The third specimen showed very obvious signs of both phenomena; but when tested was found, like iron, to lose its magnetic properties at a dull red heat instead of at the lower temperature at which pure nickel loses its magnetic properties, and therefore probably contained rather a large quantity of iron. The other two specimens lost their magnetic properties at a temperature of about 400° C. Only one specimen of cobalt was examined. Cobalt has not been drawn into wire as yet, though both Messrs. Johnson and Matthey, and Mr. W. Wiggin, jun., have kindly attempted to do so for me. Mr. Wiggin was, however, so good as to have rolled for me a specimen of cobalt, which is very nearly pure, in a strip about ½ millim. thick, 12 millim. broad, and 60 centim. long. This strip was tested with a Bunsen-burner and with a large blowpipe, and showed no trace at any temperature of either phenomenon when subjected to bending-stresses of various amounts. Unlike nickel and iron, cobalt does not lose its magnetic properties at any temperature at which it has been tested.

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[*] Sometimes there is merely a sudden check in the rate of cooling without any sensible recalescence.

[†] This is not the case as regards the phenomenon of iron suddenly losing its magnetic properties at a dull red heat.

[‡] Professor Barrett had previously shown the phenomenon to be absent in nickel.

The Working of Iron at High Temperatures.

There can be little doubt that such experiments as these are important from a technical point of view. It is known, I am not aware whether generally or not, that it is dangerous to work iron within certain ranges of temperature. This one can easily understand from what has gone before; for it is manifest that if the temperature of a mass of iron is not the same throughout, and if the *mean* temperature of the mass be near the higher of the two critical temperatures, or if it is cooling from a bright red heat near either of the two critical temperatures, the effect of a blow or of any stress will be very different on different parts of the mass, and will be fatal to that uniformity of structure and strength which it is so desirable to procure.

Summary.

(1) There are two distinctly marked critical temperatures for iron; the lower somewhere about dull red, and the higher somewhere about bright red*.

(2) At the former of these critical temperatures the iron

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* I hope at some future time to be able to fix these critical temperatures more exactly.

begins suddenly to lose its magnetic properties if it is being heated, and to gain them if it is being cooled.

(3) At or *near*[*] the latter critical temperature the iron, on being heated, if under the slightest torsional, longitudinal, or flexural stress or strain, begins to exhibit a remarkably sudden change. If the metal be under stress, the sudden change resembles that which would result from a sudden increase of elasticity. If the metal be permanently strained, there is a sudden increase in the amount of the strain.

(4) When the iron is cooling, a sudden change, but opposite in direction to that which occurs on heating, takes place. The temperature at which the sudden change takes place is, for very well annealed iron, nearly the same as that at which the change takes place on heating. If, however, the wire be not annealed, the change may be delayed by the so-called coercitive force of the metal until the lower critical temperature is reached. When the change is so delayed, the phenomenon of recalescence occurs.

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[*] It is not unlikely that the so-called coercitive force may cause the change to take place at a slightly higher temperature than the critical temperature.

PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

JANUARY 1888.

XIII. *On a Geometrical Determination of the Conditions of Maximum Efficiency in the case of the Transmission of Power by means of Alternating Electric Currents.* *By* THOMAS H. BLAKESLEY, *M.A.*[*]

1. THIS paper treats the above problem as a particular application of the method of representing Combinations of Electromotive Forces possessing a simple Harmonic Law of Change, already given by the author more than three years ago, and detailed in a series of papers published during 1885 in the 'Electrician.'

It was then pointed out that, though with two constant electromotive forces acting in a simple circuit the transmission efficiency does not exceed the ratio of the smaller to the larger, yet with alternating electromotive forces [denominated by their maximum values] the efficiency may exceed this ratio, owing to the self-induction of the circuit, and to the possibility of varying the interval of time at which the phases of one electromotive force may follow the corresponding phases of the other.

It is here proposed to show how the particular interval of time which will give the maximum efficiency may be geometrically determined, and what the value of that maximum efficiency is for two given electromotive forces undergoing harmonic repetition in the same given period, in a simple circuit possessing a known resistance and a known coefficient of self-induction. The phase-interval is then the only independent variable in the problem, and what its value must be to give the maximum efficiency of transmission of Power, and what that efficiency will then be, are the questions to which we shall have answers from Geometry alone. A short statement of the general method of representation will make the particular steps required for these problems perfectly clear.

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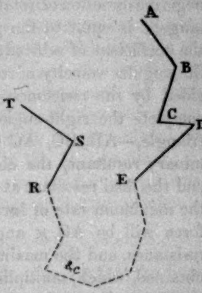
[*] Read November 12, 1887.

2. Let a straight line of fixed length, and situated in the plane of the paper, undergo uniform rotation in that plane. Then its projection upon a fixed indefinitely long line also in that plane will undergo harmonic variation, and may represent any magnitude capable of undergoing such change (*e. g.* an electromotive force), the maximum value of this varying magnitude being represented by the revolving line itself. The period in which the revolving line makes one complete revolution is the period of the change. Hence, if we know the position of the fixed line and of the revolving line at any instant, we can say in what particular phase the magnitude undergoing harmonic change is at that instant. For instance, suppose these lines make 30° with each other, we can say at once that the magnitude is removed from its maximum value by an interval of time equal to one twelfth of the period. If the angle is at the instant increasing, the magnitude has passed its maximum value that interval of time ago. If the angle is growing less, the magnitude will attain its maximum after that interval of time. It is therefore necessary to fix a positive direction of rotation as representing the positive lapse of time. [That direction which is opposite to that of the hands of a watch will here be adopted.]

3. It follows that when we have two such electromotive forces acting in the same circuit, having different maximum values but the same Period, since each is represented by the projection of a revolving straight line upon a fixed straight line, the resultant electromotive force at the instant is the algebraical sum of the individual projections. And if the two revolving lines are laid down as the two sides of a triangle *taken in order*, the rotation being uniform and the same for

both lines, the lines will remain always inclined at the same angle to each other, and the algebraical sum of their projections is the projection of the third side. Thus, in the matter of such electromotive forces, we have a theorem exactly corresponding to the triangle of directed quantities.

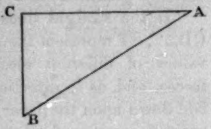
4. We may extend this mode of representing such quantities so as to form a theorem corresponding to the polygon of directed quantities, and cite it thus :—



"If the straight lines AB, BC, CD, . . . ST represent the maximum values of different electromotive forces, and, as to direction, are so laid down upon the paper that their projections upon a fixed straight line represent at some point of time the instantaneous values of those electromotive forces, their instantaneous resultant is the projection of the simple straight line AT."

5. If, in any particular case, we have taken into consideration all the electromotive forces concerned, then clearly the line representing the resultant corresponds in phase with the instantaneous current; and if by scaling or calculation we find the value of this resultant in volts, we have only to divide by the resistance in ohms to obtain the maximum value of the alternating current resulting from all the component electromotive forces. This is true even if one of the electromotive forces is that of self-induction. But suppose we have arrived at a preliminary resultant by compounding all the electromotive forces with the exception of that of self-induction ; we then require the final resultant, and we obtain it by remembering that it must be at right angles to the electromotiv force of self-induction ; for the electromotive force of self-induction must be greatest when the current is passing through zero : therefore it must have its projection on the fixed line greatest when that of the final resultant (corresponding with the current) is zero. Therefore the final resultant and the electromotive force of self-induction must be to the preliminary

resultant as the two sides of a right-angled triangle, including the right angle, are to the hypothenuse; and as we already possess the hypotheneuse we have only to determine the ratio of the sides, and upon which side of the hypotheneuse they must be placed, in order fully to determine the position and size of the final resultant and the electromotive force of self-induction. The geometrical construction is as follows.



From one end of the preliminary resultant set off an angle in the negative direction of rotation whose tangent is equal to the product of the coefficient of self-induction and the angular velocity of rotation divided by the resistance, and then complete the right-angled triangle. For if ABC is such a triangle,—AB, BC, AC representing respectively the preliminary resultant, the electromotive force of self-induction, and the final resultant at the maximum values,—it is clear that the maximum rate of increase of the resultant electromotive force will be AC × angular velocity. Divide this by the resistance, and the maximum rate of increase in the *current* is obtained, which, multiplied by the coefficient of self-induction, must give the maximum electromotive force of self-induction, from the fundamental conception of that magnitude.

Hence, in symbols, if r = the resistance,

L = the coefficient of self-induction,

ω = the angular velocity,

$$\mathrm{BC} = \frac{\mathrm{AC}}{r}\,\omega\,\mathrm{L},$$

or

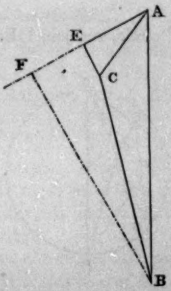
$$\frac{\mathrm{BC}}{\mathrm{AC}} = \tan \mathrm{BAC} = \frac{\omega \mathrm{L}}{r}.$$

If 2T is the period, $\omega = \frac{2\pi}{2\mathrm{T}}$, $\therefore \tan \mathrm{BAC} = \frac{\mathrm{L}\pi}{\mathrm{T}r}$.

And since the electromotive force of self-induction must be greatest and + ve when the current is changing *through zero from + ve to − ve*, it is clear that the phases of the electromotive force of self-induction must *follow* those of the final resultant electromotive force at an interval of time represented by a quarter of the period. Thus the above construction is justified.

6. The power working at any instant in a source of electromotive force is the value of the product of the instantaneous electromotive force in question and of the instantaneous current; but this is constantly changing during a period, and the mean power is half the product of the maximum value of the electromotive force, of the maximum value of the current, and of the cosine of the angle representing the time-interval between their similar phases. I have given a geometrical proof of this theorem in "Alternating Currents." It amounts simply to this in the methods of representation here employed, that if we project the revolving line corresponding to any particular source of electromotive force upon the direction of the final resultant, the power derived from this particular source will be the product of such projection and the final resultant divided by twice the resistance. Hence the various powers of the different sources will simply be proportional to the various projections upon the line of the final resultant.

7. Suppose, then, that AB, BC are the revolving representatives of two electromotive forces. Then AC is their resultant; CAE is an angle whose tangent is equal to $\frac{L\pi}{Tr}$, as explained; CE, BF are perpendiculars upon AE. Then AE is the final resultant or effective electromotive force, merely requiring division by the resistance to give the current.



The power derived from the source of AB is AF $\frac{AE}{2r}$; the power transferred to the source of BC is FE $\frac{AE}{2r}$; and the power heating the circuit is AE $\frac{AE}{2r} = \frac{AE^2}{2r}$.

As regards the projection of BC, viz. FE, since (as here drawn) FE is in a contrary direction to AE, there is a transfer of power to its source. Had F been situated nearer to A than E is, the source of BC would do work and assist in heating the circuit. This obviously depends upon whether BC, AE are inclined to one another at an angle greater or less than a right angle.

If we denominate these three powers as the power of the active source, the power of the recipient source, and the heating-power, they will be to each other in the proportion

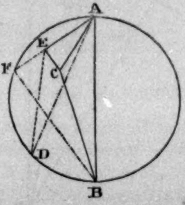
$$AF : FE : AE ;$$

and the efficiency of transmission will be $\frac{FE}{AF}$, the ratio of waste being $\frac{AE}{AF}$.

8. Since AFB is a right angle, F always lies upon the circle described upon AB as diameter. Describe this circle. We may call it briefly the F circle.

At A set off the angle BAD in the negative direction equal to CAE, *i. e.* its tangent is equal to $\frac{L\pi}{Tr}$, and therefore its magnitude is *independent* of the angle of phase-difference, $\{\pi - ABC\}$.

Let D be in the F circle, and join DE.



Now it is easy to see that the two triangles EAD, CAB are similar, and their sides homologous in the order of the letters given. For

$$EA = CA \cos EAC,$$

$$DA = BA \cos DAB = BA \cos EAC,$$

and the angle

$$EAD = EAC + CAD = DAB + CAD = CAB.$$

Hence the third side

$$DE = BC \cos EAC,$$

and therefore is invariable, whatever the phase-difference between the lines AB, BC.

But D is also fixed; therefore E lies upon a circle whose centre is D, and whose radius is BC cos EAC, or BC cos DAB. I call this circle the E circle.

We have now reached the following state of things:—

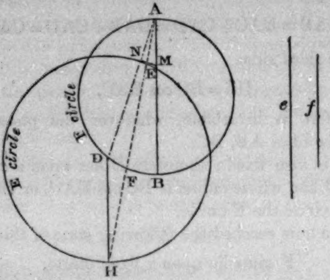
F must lie upon a fixed circle,
E " " " "
and AEF are in one straight line.

With these prefatory remarks I now proceed to the solution of the questions particularized at the beginning of the paper; which will now be seen to be merely the finding out of the particular position of E upon its circle, which will make the ratio $\frac{FE}{AF}$ as large as possible, or, which is the same thing, that of $\frac{AE}{AF}$ as small as possible.

9. Let *e* and *f* be the two electromotive forces. Take AB equal to *e*, and cut off from it BM equal to *f*. Describe the F circle upon AB, and set off BAD as before, an angle having its tangent equal to $\frac{L\pi}{Tr}$.

D is on the F circle. Draw MN perpendicular to AD, and with centre D at distance DN describe a circle. This is the E circle, for its centre is at D, and its radius $= DN = BM \cos DAB$.

Through D draw the radius DH parallel to AB, H being on the same side of D that M is of A. Join AH by a straight line cutting the E circle in E and the F circle in F. Then these are the particular positions on the circles which make the ratio $\frac{\text{FE}}{\text{AF}}$ a maximum; *i. e.* $\frac{\text{FE}}{\text{AF}}$, as here found, is the maximum efficiency. And if DE be joined, the angle ADE



will give the phase-difference between the electromotive forces to give this efficiency.

For, in the first place, it is clear that the ratio $\frac{\text{FE}}{\text{AF}}$ has some maximum (not a minimum) value between the positions where E and F coincide, viz. when the two circles intersect. And because DH is parallel to AB, and AH cuts them, therefore the angle DHE = the angle BAF.

But DH, BA pass through the centres of the two circles respectively; therefore AH, making the same angle with BA as it does with DH, must cut the circumferences of the E and F circles at the same angle. Thus at E and F, the points where AH cuts the E and F circles respectively, the arcs of those circles are parallel. Therefore an elemental displacement of E and F from these positions would result in no alteration of the ratio in which E divides AF.

But this is the characteristic of a maximum or minimum value of such a ratio. Hence the maximum efficiency is $\frac{\text{FE}}{\text{AF}}$; and it has before been proved that the triangle ADE is similar to one in which the sides homologous to AD, DE are the electromotive forces themselves. Thus, for maximum efficiency, the phases of *f* must follow the phases of *e* by an angle equal to $(180° + \text{ADE})$, or, which is the same thing, precede them by (180° − ADE) or (π − ADE). This, reduced to time, is

$$\frac{(\pi - \mathrm{ADE})}{2\pi} 2\mathrm{T} = \left(1 - \frac{\mathrm{ADE}}{\pi}\right) \mathrm{T}.$$

It is clear that $\frac{\mathrm{DN}}{\mathrm{AD}}$ is the ratio of f to e. The maximum efficiency $\frac{\mathrm{FE}}{\mathrm{AF}}$ exceeds this; and it is seen to do so in virtue of the existence of a coefficient of self-induction, the absence of which would cause D to coincide with B.

Addendum.—Analytical Expressions.

The value of the maximum efficiency in symbols is

$$\frac{f}{e}\left\{\frac{1+\frac{f}{e}\cos\beta}{\frac{f}{e}+\cos\beta}\right\},$$

where $\tan\beta = \frac{\mathrm{L}\pi}{\mathrm{T}r}$. The fraction $\frac{1+\frac{f}{e}\cos\beta}{\frac{f}{e}+\cos\beta}$ can be easily shown to be greater than unity. And the angle ADE may be calculated, if desired, as follows :—

$$\mathrm{ADE} = (2\chi - \beta),$$

where

$$\tan\chi = \frac{\sin\beta}{\cos\beta + \frac{f}{e}}.$$

[χ is the angle DHA or BAH.]

Adapting to logarithms.

Let $\frac{f}{e} = \cos\alpha$,

$$\tan\chi = \frac{\sin\beta}{2\cos\frac{\beta+\alpha}{2}\cos\frac{\beta-\alpha}{2}}.$$

There may be transmission of power from the source of e to the source of f even when $f > e$, provided that $f\cos\beta$ is not $> e$; as would appear at once from a geometrical construction on the plan given above, and in any case the condition of maximum efficiency is one of stability.

XIV. *On a Method of Discriminating Real from Accidental Coincidences between the Lines of different Spectra; with some Applications.* *By* E. F. J. Love, *M.A., Demonstrator of Physics in the Mason College, Birmingham.*[*]

[Plate VI.]

In investigating problems the solution of which depends on the coincidence of the lines belonging to different spectra, *e. g.*, in determining the presence of any particular substance in the Sun's reversing layer, it is usually considered sufficient to demonstrate a close agreement between the wave-lengths of a number of lines in the spectra. That this method, however, taken by itself, does not suffice to give us a reliable result was shown by Schüster[†], who demonstrated that, in accordance with the Theory of Probability, a certain number of coincidences between the lines of two spectra might be expected to occur, even if the spectra be quite unrelated; and showed how to calculate the maximum number of coincidences possible on the assumption that no relation exists between them. If no greater number than this is found, the coincidences must be looked upon as probably accidental. The method as given by Schüster is employed to determine whether the lines of a spectrum are harmonically related; but it could obviously be quite as well applied to examine the relations between two different observed spectra. It demands, however, a considerable amount of rather troublesome computation. Further, cases may occur in which approximate equality obtains between a great number of lines in two spectra, one or both of which is so crowded with lines that the question arises, What difference of wave-length between the lines is admissible as a coincidence? This difficulty especially meets us when we are dealing with certain parts of the solar spectrum—especially those obtained from portions of the sun's surface near to, or including, a spot; and Schüster's method gives us no hint towards its solution.

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[*] Read November 26, 1887.

[†] Proc. Roy. Soc. xxxi. p. 337, 1881.

Fig. 1.

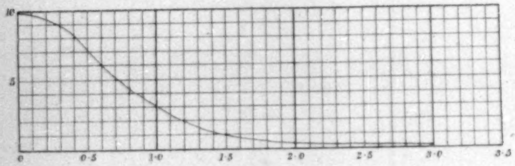


Fig. 2.

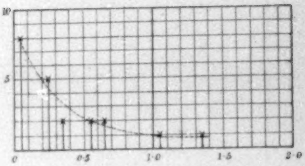


Fig. 3.

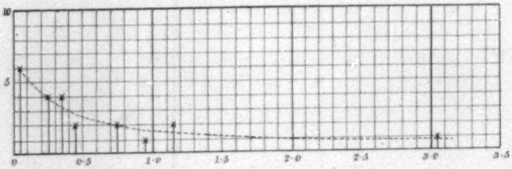


Fig. 4.

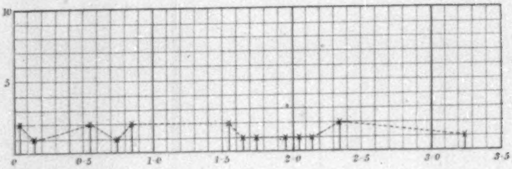


Fig. 5.

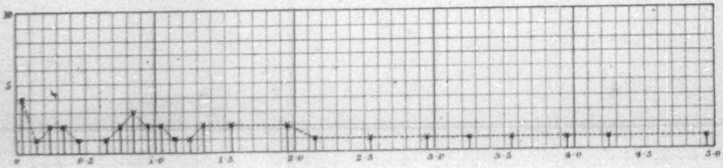


Fig. 6.

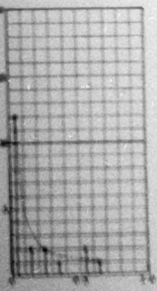


Fig. 7.

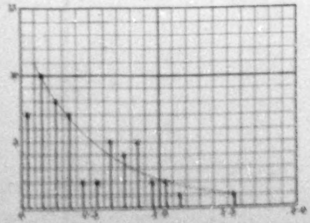
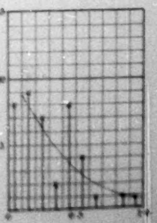


Fig. 8.



[illegible]

While engaged in preparing a report on Grünwald's* recent investigations into the relation between wave-length and specific volume, the present writer was led to a simple method of comparison, based on the Law of Error. In accordance with this law, the errors of observation of a single quantity group themselves about the mean value of the quantity in such a way that the number of observations in which the errors are less than some small quantity x is greater than the number in which they lie between x and $2x$, this again is greater than the number between $2x$ and $3x$, and so on; the equation between the number of observations and magnitude of error being, as is well known, of the form

$$y=a\epsilon^{-c^2x^2}.$$

Now since the various spectrum-lines of a substance in a given physical condition are connected by an invariable relation, it seems allowable to assume that observations of the several lines in one spectrum may be regarded as different observations of one phenomenon, viz., that spectrum; as a consequence it is here assumed that, if the differences of the wave-lengths of corresponding lines in spectra really due to the same substance, but determined by different observers, and under different conditions (*e.g.*, the substance as examined by one observer being on the earth; and as examined by the other, on the sun) be compared, they will accord with the Law of Error. The method thence derived is as follows:— The differences between the wave-lengths of the lines compared are arranged in groups, each group containing those observations the errors of which lie within certain narrow limits. The number of observations in each group is then plotted as an ordinate of a curve, the average error of the group being the abscissa. If this curve be then compared with the curve given by the Law of Error, any serious divergence from the form of the latter curve is at once made manifest. It should, however, be borne in mind that the Law of Error admits the possibility of errors of every conceivable magnitude, and assumes the number of sources of error to be practically infinite; as a result we should expect the curves actually obtained to be steeper if anything than the theoretical curve in the portion near the y-axis.

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* *Astr. Nachr.* Bd. 117; Phil. Mag. [5] xxiv. p. 354, 1887.

It is obvious that the method can only be applied to spectra which contain a considerable number of lines, and that measurements of *all* the coincidences observed must be included, otherwise the method will not give a correct result; it might under different circumstances cause us either to underrate or overrate the probability in favour of the coincidence. On the other hand, in dealing with the solar spectrum with the aid of Ångström's map, it must be borne in mind that the map is very incomplete, many lines being omitted; as a result we must not expect to find *all* the lines of any substance in the map, even if these lines exist in the sun.

We may illustrate the method by means of the curves shown in figs. 1–5 (Plate VI.). Fig. 1 is the Theoretical Curve of Error; fig. 2 that actually obtained by comparing the values of 21 lines in the arc-spectrum of iron, observed by Ångström, with their values obtained by Cornu; fig. 3 is the curve obtained on comparing Kirchhoff's measurements of the spark-spectrum of cerium (27 lines) with those of Thalén. The agreement in form between these curves is obvious, and, considering the somewhat small number of lines included, rather striking; the greater steepness of the two latter curves (owing to the finite number of sources of error) is also well marked. Fig. 4 contains a comparison of the arc-spectra of iron and nickel (19 lines) between wave-lengths 4850 and 5890; and fig. 5 a comparison of those of iron and titanium (34 lines) between the same limits. The divergence of these from fig. 1 is, as we should naturally expect, very marked; so far as *any* curves may be said to represent the results, the best would be straight lines nearly parallel to the x-axis.

Having given these instances as illustrations of the degree of accuracy of the results to be obtained from the method, we will proceed to examine its bearing on two problems of considerable interest: (1) The existence of cerium in the sun; (2) Professor Grünwald's recent investigations.

Cerium in the Sun.

The existence of cerium in the sun's reversing layer was indicated as probable by Professors Liveing and Dewar* in 1882; and in the following year the same observers† published a map, extending from F to *b*, showing among others the arc-spectrum of cerium, and the spectra of the lines seen widened in two sun-spots at Greenwich in 1881. The spectrum of cerium exhibits numerous coincidences with widened lines, though comparatively few with lines given in Ångström's map. On the other hand, Messrs. Hutchins and Holden[*], who have re-examined the evidence for the existence of certain substances in the sun, with the aid of photography, remark, ‘So numerous are the lines [of cerium, molybdenum, uranium, and vanadium] that often on the photographs the total space occupied by them is greater than the space not so occupied. . . Evidently coincidences between these and solar lines cannot fail to occur as matters of chance and therefore prove nothing. One can easily count a hundred or so such coincidences without the slightest conviction that the connexion is other than fortuitous.”

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* Proc. Roy. Soc. xxxiii. p. 428, 1882

† Phil. Mag. [5] xvi. p. 401, 1883.

As a complete map of the arc-spectrum of cerium has not yet been published, the writer has been obliged to fall back on the evidence given by Liveing and Dewar's map, mentioned above. It is briefly as follows:—Of the 34 lines included in the map 20 coincide with solar lines which are not already assigned or possibly assignable to other metals; of these solar lines 6 only are represented on Ångström's map, and *the whole* have been observed to be widened, most of them very considerably, in sun-spots, though not all in the same spot. This fact by itself considerably strengthens the case for the existence of cerium as a constituent of the sun, since it demonstrates a connexion between these lines. Let us apply the method to these twenty coincidences. The differences between the wave-lengths of the cerium and solar lines are given in the following table:—

Between 0·0 and 0·1 Xth metre, 12 coincidences.
„ 0·1 „ 0·2 „ „ 2 „
„ 0·2 „ 0·3 „ „ 2 „
„ 0·3 „ 0·4 „ „ 1 „
„ 0·4 „ 0·5 „ „ 0 „
„ 0·5 „ 0·6 „ „ 2 „
„ 0·6 „ 0·7 „ „ 1 „

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[*] Proc. Amer. Acad. Arts and Sci. xxiii.; Phil. Mag. [5] xxiv. p. 325, 1887.

These numbers when plotted give the curve represented in fig. 6. This curve closely resembles figs. 1, 2, and 3, except in respect of its greater steepness; the latter, so far from being an objection, testifies to the fewness of the sources of error affecting the wave-length measurements. The evidence afforded by the method appears then to supply a substantial confirmation to the reality of the coincidences, so far as the material for investigation goes.

Professor Grünwald's Investigations.

The most important verification of his theory put forward by Grünwald is that afforded by the close agreement between the wave-lengths of the lines in the spectrum of water, as deduced by him from those of the hydrogen spectrum, and their values as obtained by observation. So far, 58 of the predicted lines have been observed by Prof. Liveing; and the remainder occur in a part of the spectrum not yet submitted to examination. The wave-lengths are given in Grünwald's[*] paper; the differences between the observed and calculated values are given here:—

Between 0·0 and 0·1 Xth metre, 7 coincidences.
" 0·1 " 0·2 " " 10 "
" 0·2 " 0·3 " " 8 "
" 0·3 " 0·4 " " 7 "
" 0·4 " 0·5 " " 2 "
" 0·5 " 0·6 " " 2 "
" 0·6 " 0·7 " " 5 "
" 0·7 " 0·8 " " 4 "
" 0·8 " 0·9 " " 5 "
" 0·9 " 1·0 " " 2 "
" 1·0 " 1·1 " " 2 "
" 1·1 " 1·2 " " 1 "
... .
" 1·5 " 1·6 " " 1 "

These when plotted give the curve fig. 7, which agrees almost exactly with the formula for the Theoretical Curve of Error, as tested by taking points. On the other hand, four of the points do not agree so well with the smooth curve as those in the cases already examined; and until further measurements are obtained, the method only warrants us in asserting that the balance of probability lies on the side of the reality of the coincidences.

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[*] Phil. Mag. [5] xxiv. p. 357, 1887.

The curve shown in fig. 8 is extremely interesting; it is plotted from the comparison, in Grünwald's paper, between the spectrum of one of his hypothetical constituents of hydrogen, termed by him "*b*," and the nearest solar lines, as given in Ångström's map. The number of lines included is 41. The differences are as follows:—

Between	0·0	and	0·1	Xth metre,		8	coincidences.
„	0·1	„	0·2	„	„	9	„
„	0·2	„	0·3	„	„	7	„
„	0·3	„	0·4	„	„	2	„
„	0·4	„	0·5	„	„	8	„
„	0·5	„	0·6	„	„	4	„
„	0·6	„	0·7	„	„	1	„
„	0·7	„	0·8	„	„	0	„
„	0·8	„	0·9	„	„	1	„
„	0·9	„	1·0	„	„	1	„

Here again two points are very much out; but an over-estimation of $\frac{1}{10}$th of a Xth metre (a quite possible mistake) in reading off Ångström's scale at three different places would replace these two points in the curve; we may therefore fairly consider that the method affords support to Grünwald's hypotheses.

A curious point in connexion with figs. 7 and 8 lies in the fact that in both the first experimental ordinate, instead of being the greatest, is smaller than the second. Is it possible that this may indicate a systematic error in Grünwald's calculations? The probability of this seems increased by an examination of the errors with regard to sign; for, in the comparison of the hydrogen and water spectra, the average positive error (obtained by dividing the sum of all the errors in which the predicted exceeds the observed value by the number of such errors) is $\frac{13\cdot6}{22}=0\cdot6$ Xth metre; while the average negative error is $\frac{17\cdot2}{33}=0\cdot5$ Xth metre. Since a constant arithmetical error is highly improbable, the only explanation of this seems to be a small systematic difference between Hasselberg's scale of measurement for the hydrogen-spectrum (from which Grünwald's water lines are obtained by halving the wave-lengths of the hydrogen-lines) and Ångström's scale. As it would seem to be very difficult to detect such a difference of scale in any other way, this example adds another to the purposes to which this method may be applied.

XV. *The Effect produced on the Thermoelectrical Properties of Iron when under Stress or Strain by raising the Temperature to Bright-red.* *By* HERBERT TOMLINSON, *B.A.*[*]

THE author has already had the pleasure of communicating to the Physical Society[†] the results of some experiments relating to a certain curious behaviour of iron wire when under stress or strain at a temperature of bright red. It was found that if the metal was under a slight stress either of torsion or of flexure, there was *apparently* a sudden increase of elasticity when, on heating, the critical temperature had been reached; that is to say, if the wire was under a slight twisting stress it suddenly untwisted, and if under a slight bending stress it suddenly unbent. Corresponding sudden changes in the opposite direction occurred on cooling. When, however, the wire was permanently strained and relieved from stress the equally sudden changes which ensued at the critical temperature were opposite in direction to the changes produced when the wire was under stress; namely, a permanently twisted wire free from torsional stress would suddenly twist further when, on heating, a temperature of bright red was reached, and a permanently bent wire would suddenly bend further. Likewise the sudden changes which took place on cooling were in opposite directions for stress and permanent strain: the permanently twisted wire suddenly untwisted, and the permanently bent wire unbent.

Those sudden changes were regarded as probably indicating equally sudden changes in the molecular architecture of iron; and experimental evidence was brought forward to prove that the temperature at which the above-mentioned phenomena occurred was not the same as that at which iron suddenly loses its magnetic properties.

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[*] Read November 26, 1887.

[†] *Ante*, p. 67.

Again, Sir W. Thomson* arrived at the remarkable conclusion that when a permanent strain is left after the withdrawal of the stress producing it, the residual *thermoelectrical* effect is the reverse of the thermoelectrical effect which is induced by the stress and which subsists as long as the stress acts.

The author was led by the above-mentioned considerations to attempt to ascertain how far the thermoelectrical properties of iron when under temporary stress or permanent strain might be affected by raising the temperature to a bright red.

Experiment I.

A piece of well-annealed iron wire one millimetre in diameter was subjected throughout half its length to a great many turns of permanent torsion. It was then supported on the ring of a retort-stand, being insulated from the latter by paper, with the junction of the twisted and untwisted portions in the centre of the ring. The free ends of the iron wire were connected by silk-covered copper wire with the terminals of a delicate reflecting-galvanometer of about 7 B.A. units resistance; the two junctions between the iron and copper being tied together and well wrapped up in tissue-paper, which served also to insulate them from each other. The junction of the twisted and untwisted portions of the iron wire was now heated slightly by a Bunsen's burner; this caused a deflection of the galvanometer, indicating a current from the strained to the unstrained portion through the hot junction, as already found by Sir William Thomson. The flame of the burner, which was placed underneath the junction, was at first kept small, but was afterwards increased by degrees. At about dull red temperature the deflection nearly ceased to increase, but as soon as the wire assumed a bright red colour, the light reflected from the mirror of the galvanometer went off the scale. The adjusting magnet was therefore used to bring the spot of light back on to the scale in the hope of getting a fairly steady deflection; but this was found to be impossible, the light frequently changing its position by fits and starts and sometimes going over to the other side of the scale. The reason of this unsteadiness will be explained later on.

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* "Electro-dynamic Qualities of Metals," Phil. Trans. Part IV. 1856.

The experiment was repeated with a second piece of the same wire similarly treated, with this difference, however, that after slightly heating the junction to observe the direction of the deflection, a large resistance was introduced into the galvanometer circuit. By this means the variations of the sudden deflection again produced at the temperature of bright red were much diminished, and a moment of fair steadiness was seized to remove the burner and allow the wire to cool. Almost immediately after the removal of the burner the light on the scale, *after pausing slightly*, went suddenly in the opposite direction.

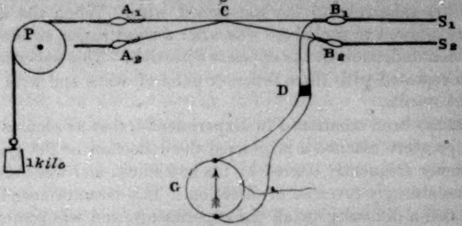
The experiment was repeated some six or seven times with different pieces of the same wire, but always with like results.

Experiments similar to the above were tried with other pieces of the same iron wire, to which permanent strain was imparted by hammering transversely, by stretching longitudinally, or by bending the wire into coils of small diameter. In all these experiments the thermoelectrical currents produced by moderate heating were, as Sir W. Thomson has already found, from the strained to the unstrained through the hot junction, and in all there was a very sudden increase of the current when, on being heated, the wire became bright red, and an equally sudden decrease when the wire was cooled.

Experiment II.

Two pieces of the same wire were arranged as in fig. 1. A_1 B_1, A_2 B_2 are two wires brazed together at C and looped at A_1,B_1 and A_2, B_2; from B_1 and B_2 the wires continue to D, where they are joined to the connecting wires of the galvanometer G; D was well wrapped up in tissue-paper. B_1 S_1, B_2 S_2 are pieces of strong string fastened at S_1, S_2 to the ring of a retort-stand weighted at the bottom. From A_1 passes a string over a pulley P, and to the end of this string a weight of 1 kilo. was attached ; from A_2 passes another string to a support so as to hold up the portion A_2C of the wire A_2, B_2. The two wires A_1 B_1, A_2 B_2 lie in the same horizontal plane ; to the left of C they are covered with sand from C to a distance of 10 centims. To the right of C for a distance of about 3 centims. the wires are bare, but beyond this for about 7 centims. are covered with sand ; the sand is sustained on two stands placed to the right and left of C.

Fig. 1.



First, the junction C was slightly heated by a burner, and as the weight of 1 kilo. was not sufficient for the moderate temperature then used to produce any sensible thermoelectrical effect, the end A_1 was pulled rather hard by the hand to ascertain what would be the direction of the thermoelectrical current due to the stress ; this direction was found to be from the unstressed to the stressed wire through the hot junction, as Thomson had already found. A rather large resistance was then introduced into the galvanometer circuit for the purpose of diminishing the sensibility of the instrument, and the parts of the uncovered wires to the right of C were raised to the temperature of bright red. Immediately this temperature was reached, there was a sudden start of the galvanometer-needles, indicating that the stretched wire had suddenly become more *negative* relatively to the unstretched wire. When the wires were allowed to cool there was, after a short pause, an equally sudden deflection in the opposite direction.

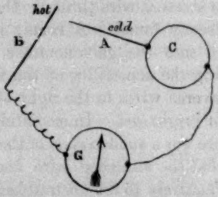
The weight of 1 kilo. was now supported by blocks placed underneath it, and the junction C was again heated to a bright red. At the critical temperature there was again a sudden deflection, now indicating that the wire, which had been previously permanently lengthened by the 1 kilo. weight at the bright red temperature, was rendered suddenly more *positive* relatively to the unstrained wire. When the wires were allowed to cool there was, after a short pause, an equally sudden deflection in the opposite direction. This experiment was repeated with three different pairs of wires and with the same results.

It has been mentioned in Experiment I. that as soon as the temperature reached a bright red the deflection of the galvanometer frequently altered by fits and starts, and was even at times strongly reversed in direction. This circumstance constituted a difficulty in all the experiments, and was presently found to be due to slight shiftings of the flame of the burner from slight draughts in the room. Hot iron at any temperature is thermoelectrically negative to cold iron; but when the temperature reaches a bright red there is such a sudden accession of negativeness that if we lay a piece of iron at this temperature upon another piece of cool iron there is developed an electromotive force which, thermoelectrically considered, is very large.

Experiment III.

In fig. 2 A is a piece of the same iron wire connected with one terminal of a condenser C having a capacity of one third

Fig. 2.



of a microfarad; B is a second piece of the same iron wire connected by insulated copper wire with one terminal of a reflecting-galvanometer G having a resistance of about 6,000 B.A. units; the other terminals of the condenser and galvanometer are connected together with insulated copper wire. The free end of B was heated in a burner nearly to a white heat, and then, having first been removed from the flame, was quickly placed on the free end of A; immediately there was a deflection of the spot of light on the galvanometer-scale through 10 divisions, showing that the condenser was being charged. Almost immediately after B had touched A, it was removed, and after allowing sufficient time for the wires to cool B was again placed on A, when a deflection through 10 divisions of the scale in the opposite direction indicated the discharge of the condenser. This experiment was repeated several times with fairly uniform results; and by comparing the deflection thus obtained with that produced by a Daniell's cell, it was ascertained that the E.M.F. produced by putting iron at a bright red temperature on iron at about 15° C. was one twentieth of a volt, *i. e.* considerably more than twice as much as would be developed in a single element of bismuth and antimony with one junction at 100° C. and the other at 0° C.

It has long been known that hot iron is negative to cold iron, and this seems to be so at all temperatures, but it has not, the author believes, been noticed before* how very suddenly the negativeness is increased when the temperature reaches a bright red. If the wire B be heated to a temperature between dull red and bright red before being placed on A, there is a current from A to B which can be readily detected if the condenser be cut out and the galvanometer connected directly with A and B, but no effect whatever could be detected when the condenser was used as in the experiment.

Experiment IV.

A piece of the same iron wire was connected directly with the two terminals A, B of a reflecting galvanometer of 7 B.A. units resistance (fig. 3). A portion C D of the wire was heated above a bright red by a burner, and as soon as the required temperature had been reached the burner was shifted suddenly and slightly towards C; almost immediately afterwards the light darted off the scale of the galvanometer, indicating a strong current from A to D through C. The

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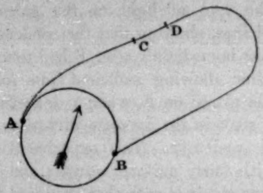
* Unless we except Prof. P. G. Tait's observations, which will be referred to presently.

Fig. 3.



portion C D was again heated above a bright red temperature, and the burner then was shifted suddenly towards D; the deflection of the galvanometer-needle was now strong in the opposite direction, showing a current from B to C through D.

Experiment III. furnishes the clue to the explanation of the phenomenon described in Experiment IV. The portion C D, when at a bright red, is thermoelectrically negative to the rest of the iron wire, and is in fact like a different metal. So long, however, as the flame of the burner is kept in one position the temperature at C will be the same as at D, and there will be no current; but directly we shift the burner towards C we make C hotter than before, whilst D cools more or less, and we get a current from A to D through C.

We also see why a slight draught of air blowing the flame of the burner towards C or D may suddenly generate a strong current in one direction or the other and may completely mask the phenomena recounted in Experiments I. and II.

Experiment IV. may be varied in several ways, thus:—If we keep the flame steadily burning so as to maintain C D at a high temperature, and allow a small stream of water to flow down on C, we get a continuous current from B to C through D, and if we shift the stream to D we reverse the current. Or, instead of the stream of water, we may blow with a small pair of bellows at C or D.

What connexion there may be between the above-mentioned phenomena and the discovery of Professor P. G. Tait that the thermoelectrical diagram of iron cuts that of iridium-platinum at least *three times* below a low white heat*, that is to say that an iron and iridium-platinum circuit has at least *three* neutral points, does not quite appear.

In conclusion, it may be said that the investigation serves still further to confirm the author in his opinion that a sudden and profound change takes place in the molecular arrangement of iron at a temperature of bright red.

XVI. *The Recalescence of Iron.*

By HERBERT TOMLINSON, *B.A.*†

Introduction.

MANY specimens of iron and steel, when they have been raised to a white heat and are cooling, exhibit at a certain stage a remarkable phenomenon—the metal, to all appearances, receives a sudden accession of heat, and reglows. This phenomenon was discovered by Professor Barrett, and is frequently designated the "recalescence" of iron. The author believes the recalescence of iron to be due to a sudden *physical* change, the event of which has been retarded by what he will call "subpermanent retentivity." The retentivity of a substance may be defined to be that property by virtue of which the substance does not *immediately* recover from the strain produced by a stress when the stress has been removed. If the original strain produced by the stress be large, the residual strain will consist of two parts: one permanent, which will not disappear even after the lapse of any length of time; the other subpermanent, which will disappear after a greater or less interval of time, and which may be aided in doing so by molecular vibrations set up by mechanical or any physical agency. Subpermanent retentivity appears with a great variety of strains:—when a wire is twisted or bent, when a piece of iron or steel is magnetized, when a Leyden jar is charged, and when glass has been raised in temperature. The last is an example of subpermanent strain resulting from thermal agency, and is the strain with which we have now to deal. Such a strain is not peculiar to glass; for the author has found that a coil of zinc wire, which had been previously heated to nearly the melting-point of the metal and afterwards suddenly cooled by plunging into cold water, continued to show decrease of volume for some minutes after the cooling. The internal friction of a metal seems to be intimately connected with the subpermanent retentivity; and the author has already investigated the internal friction of several metals at various temperatures*. The results of the above-mentioned investigation show that changes of temperature, even when they do not exceed 100° C., exercise a very considerable effect on the internal friction; and it seemed likely that a preliminary examination of the internal friction of iron at very high temperatures might be of service in elucidating the problem of recalescence.

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* Heat, by P. G. Tait, § 190. Also Trans. R. S. E. 1873, for similar peculiarities in nickel at lower temperatures. The author has failed to observe in nickel wire any such phenomena as those mentioned in Experiments I. and II.

† Read December 10, 1887.

The Internal Friction of Iron at High Temperatures.

Experiment I.—In fig. 1, *ab* is an iron wire 49·5 centim. in length and ·041 centim. in diameter; the ends *a* and *b* are brazed to two pieces of rather stout brass wire; the upper piece of brass wire is furnished with a binding-screw, S, and the lower one is soldered to the centre of a hollow horizontal brass bar, V; D is a piece of copper wire soldered at its upper extremity to V, and dipping with its lower extremity into mercury contained in a glass vessel, H. The wire hangs in the axis of a glass tube, T, fitting into the cover of a box, B; the tube is closed with a cork, C, and the box B, though provided with a door which can be opened for making adjustments or for starting the torsional oscillations, is kept shut up during the actual observations. The box is also provided with a glass window, which enables an illuminated circle crossed by a fine vertical dark line to be thrown on to the light mirror, M, and hence be reflected back on to a scale according to the usual arrangement. The wire was set in torsional oscillation by means of a light feather gently pressed against one end of V and then removed[*]; the amplitudes of the vibrations were never allowed to exceed one degree.

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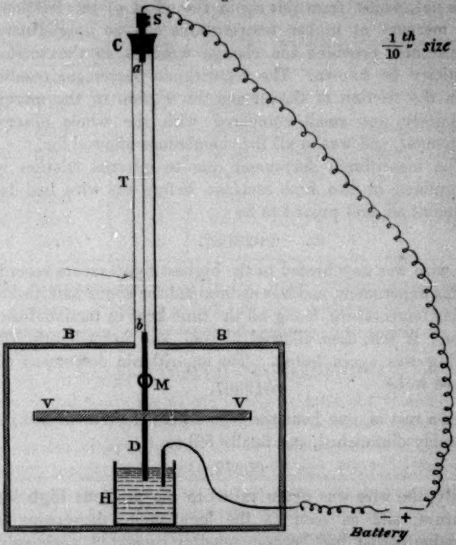
* Phil. Trans. vol. clxxvii. (part ii. 1886) p. 801. Proc. Roy. Soc. No. 244 (1886), p. 343.

Fig. 1.



The wire was heated by means of an electric current from a battery of 30 Grove cells, an ampere-meter and a set of resistances serving respectively to measure and to vary the strength of the current. The internal friction of the iron was measured by the logarithmic decrement of arc of the wire when set in torsional oscillation. Part of the whole observed decrement is due to the friction of the air, and part to the friction of the wire D against the mercury in H. The former of these can be calculated from formulæ given in the author's paper on the internal friction of metals[*]; the latter by observing the logarithmic decrement at the temperature of the room, first when D dips into the mercury, and again when it does not, whilst from this again the effect of the friction of the mercury at higher temperatures can be approximately determined provided the change wrought in the torsional elasticity be known. The logarithmic decrement resulting from the friction of the air and the friction of the mercury conjointly was small compared with the whole observed decrement, and was in all the calculations allowed for.

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[*] It would have been better to have had a small piece of iron attached to V and to have started the vibrations by a magnet on the outside of the box; since the amplitudes diminished very rapidly at the higher temperature.

The logarithmic decrement due to internal friction was determined in the first instance before the wire had been heated at all, and proved to be

·002203[†].

The wire was now heated to the highest temperature recorded in this experiment, and was maintained for about half an hour at this temperature, being all the time kept in torsional oscillation; it was then allowed to cool, and immediately after cooling was again tested. The logarithmic decrement now proved to be ·019367.

After a rest of one hour the logarithmic decrement had considerably diminished, and finally fell to

·00977.

Lastly, the wire was again raised to the previous high temperature, and as soon as the logarithmic decrement and vibration-period had become sensibly constant[‡] their values were registered. The temperature was then lowered by putting resistance into the battery-circuit, and a fresh set of observations was taken, and so on, until eventually the temperature of the wire was reduced to that of the room, when the logarithmic decrement proved to be sensibly the same as the one last recorded. The results of the observations, which extended over a period of several days, are given in the following table :—

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[*] Phil. Trans. vol. clxxvii. (part ii. 1886) pp. 813–815.

[†] The wire was unannealed, otherwise the logarithmic decrement would have been probably less than half of the amount here given.

[‡] Any change of temperature, whether in the direction of increase or decrease, is always attended in the first instance with a larger internal friction than that which ultimately prevails when the vibrations are continued long enough.

Time of vibration, in seconds.	Modulus of torsional elasticity, in grammes per square centim.	Temperature, in degrees Centigrade, t.	Logarithmic decrement due to internal friction, λ.	Difference between consecutive temperatures, Δt.	Difference between consecutive values of λ, $\Delta\lambda$.	$\Delta\lambda : \Delta t$.
1·523	$726{\cdot}6 \times 10^6$	20	·00977			
1·573	681·1	220	·01805	200	·00828	·000041
1·700	583·2	543	·05033	323	·03228	·000100
2·020	413·1	930	·09634	387	·04601	·000119
2·100	382·2	989	·11216	59	·01582	·000268
2·486	272·7	1181	·20950	192	·09734	·000507
2·600	249·4	1220	·19680	39		

The temperatures given in the third column of the table are deduced from the values of the modulus of torsional elasticity, given in the second column, by means of the formula

$$z_t = z_0(1 - {\cdot}0002442\,t - {\cdot}0000002510\,t^2)^*,$$

where z_t and z_0 are the values of the modulus at $t°$ C. and 0° C. respectively. This formula was itself deduced from some very careful observations of the torsional elasticity of iron at different temperatures between 0° C. and 100° C.; but the numbers given in the third column must only be regarded as rough approximations to the true values; inasmuch as in the first place the above formula was calculated from observations made with a different specimen of annealed iron, and in the second it by no means follows that the formula would apply for such high temperatures as are here recorded. It would seem, however, from the last column of the table, that somewhere near 550° C. there begins to be a rapid rise in the internal friction of the iron, and another still more rapid rise at a temperature of about 1000° C. At this last temperature the internal friction becomes so great that only two or three vibrations are made by the wire before it comes practically to rest; and it is necessary to multiply the observations very considerably before any approach to accuracy in the results

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[*] Proc. Roy. Soc. No. 244 (1886), p. 343.

can be attained. Some idea of the enormous friction encountered by the molecules may be arrived at by bearing in mind that the logarithmic decrement of a torsionally vibrating iron wire at 1000° C. is *ten times* the logarithmic decrement of a tin wire at a temperature of 20° C., tin possessing the largest internal friction of any metal yet examined at ordinary temperatures.

From the last line in the table it would seem that the internal friction of iron begins to decrease as the temperature rises from 1100° C. to 1200° C.; but the numbers here given must be received with some caution in so far as they are to be used for determining this point[*].

We tread on surer ground when we compare the second and fourth columns together; and it is abundantly evident that the decrease of torsional elasticity which ensues on rise of temperature bears no comparison with the increase of internal friction. It is a common lecture-experiment to heat a bell by means of a burner placed underneath, and to demonstrate that all musical sound departs from the bell when struck provided the temperature approach a visible red. This absence of musical sound is generally, if not always, attributed to diminution of elasticity; but it would seem, from the above, to be much rather attributable to increase of internal friction[†].

Another point to be noted in this experiment is the large permanent increase of internal friction produced by the rise of temperature. This doubtless arose from the fact that, though the mass of the suspended system only amounted to 40 grms., there was a sensible permanent lengthening of the wire resulting from the rise of temperature; for the author has shown that permanent extension and permanent torsion may both increase very considerably the value of the logarithmic decrement.

An equally important point to the above is the effect of rest on the logarithmic decrement when the wire has recently had its temperature changed. It seems not improbable that, large as the internal friction is at the higher temperatures, it would be larger if tested *immediately* after the change had taken place. Again, it would be larger still if tested by a statical method; for the preliminary vibrations, to which the wire was subjected before testing, evidently reduced considerably the internal friction.

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[*] A second set of experiments made with another piece of the same wire showed, however, a similar decrease of internal friction but at rather a lower temperature.

[†] The elasticity of iron at 1000° C. is nearly the same as the elasticity of silver at 20° C.

The Two Critical Temperatures of Iron.

The above experiment seems to show, not only that the internal friction of iron is very considerable at high temperatures, but also that there are two points, namely about 550° C. and 1000° C., at which there are very sudden changes in the rate at which the friction increases with the temperature. Now at or near these points there are, at any rate for annealed iron wire of good quality, very notable and sudden changes in certain of the physical properties of the metal. About 550° C. the metal begins to lose its magnetic properties very rapidly; so rapidly indeed that, if an iron wire be surrounded by a magnetizing solenoid always kept in action, and this again be concentric with a secondary solenoid connected with a galvanometer, a very sensible induced current can be observed at the critical temperature both on heating and cooling. It would seem likely also, from the researches of Prof. P. G. Tait and others, that at this temperature, or *about* this temperature, there are sudden changes in the thermoelectrical properties of the metal and also in the electrical resistance. These sudden changes would appear to indicate an equally sudden change in the molecular architecture of the iron; and it seems reasonable to suppose that when, on heating, the critical temperature is reached, a certain amount of the whole thermal energy which may be imparted by an electric current or a burner to the wire is used up in producing this change, whilst an equal amount of thermal energy is given out when the wire, on cooling, reassumes its original molecular structure. It by no means follows, however, that the so-called latent heat connected with the sudden change can be detected unless by very refined apparatus; for it is not at all unlikely that the sudden loss of magnetic properties may be due simply to a rotation of the molecules about their axes; and the researches of

Professor Ewing* prove that there may be considerable rotation of the molecules about their axes, and yet at the same time very minute change of temperature resulting from this rotation.

Near the higher temperature of 1000° C. there is again a most remarkable alteration in the behaviour of an annealed iron wire which is being heated when under stress or strain. If it is stretched by a slight weight it suddenly unstretches †; if it is under a slight bending-stress it suddenly unbends; and if it is under slight twisting-stress it suddenly untwists; whilst, on the contrary, if it has been previously bent or twisted permanently and then entirely released from stress, it suddenly bends more or twists more as the case may be ‡. Further, there are sudden changes, evidently at the same temperature, in the thermoelectrical relations which exist between strained and unstrained iron, and also between stressed and unstressed iron; these changes, like those before mentioned, being opposite in direction for stress and strain §. All these phenomena indicate, according to the author's belief, a second notable change in the molecular architecture of the iron; they certainly seem to indicate a change which must involve the expenditure of thermal energy to produce it when the iron is being heated, and the giving out of thermal energy when the iron is being cooled: in other words, here must be, one would think, a second point at which heat becomes latent.

Now suppose that the iron has been raised in temperature above 1000° C. and is cooling: when the critical temperature is reached the molecules would begin to come back to their original positions, just as water begins to freeze when the temperature of 0° C. has been reached; but internal friction prevents this, and it is not till the temperature has fallen, it may be very considerably, below the critical temperature that the readjustment takes place. When, however, the change under these conditions does take place it will be what Clerk Maxwell has designated as an explosive change, and the energy set at liberty by the transformation will accelerate the subsequent rate of transformation. Here, again, we have an analogy in the case of freezing water; for in a Wollaston's cryophorus the temperature of the water may frequently be reduced several degrees below 0° C. before solidification sets in; but when it does, the whole mass, or a considerable portion of the whole mass, is frozen instantly. With both iron and water, therefore, there will be a greater amount of heat *suddenly* generated when the transformation takes place below the critical temperatures than when it occurs at the critical temperatures. Again, another analogy may be pointed out between ice at 0° C. and iron at 1000° C.; in both there is a marked change in the cohesion of the molecules. When an iron wire is under any but an exceedingly small stress of bending, torsion, or traction, it begins to yield permanently in a most astonishing manner when, on heating, the temperature reaches 1000° C.; the metal suddenly becomes, as it were, in a partially fluid condition.

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* Phil. Trans. part ii. p. 553 (1885).

† Phil. Mag. [4] vol. xlvi. p. 472.

‡ *Ante*, p. 71.

§ *Ante*, p. 100.

According to the author's view, then, recalescence in cooling iron is owing to retardation, produced by internal friction, of a *physical* change somewhat resembling the change which occurs when water becomes ice: were it not for internal friction the change would take place comparatively in a gradual manner, and a certain amount of thermal energy would be equally gradually given out. In consequence, however, of internal friction the change is retarded until a temperature is reached below the temperature at which it would otherwise occur; when, partly owing to diminution of internal friction and partly to increased internal molecular stress, the molecules give way at some one point, and this is followed by a giving way throughout the whole mass; the change thus partakes of the nature of an explosion, and, as a consequence, there is a rapid rise of temperature. The following experiments were made with the object of collecting evidence for or against the above-mentioned theory.

Experiments on Recalescence.

It has been observed that there are *two* critical temperatures—one at about 550° C., and the other at about 1000° C.;

so that, according to the above theory, there should be at least *two* points at which a sudden *reheating* takes place; but not necessarily two sensible *reglows*[*], or even, as we have seen, two *sensible* reheatings. This is all the more to be expected; because when, on heating, a wire under moderate stress has attained a temperature of about 550° C., there is evidently a sudden *permanent* yielding showing sudden softening of the metal, though this is not nearly so marked as the permanent yielding which takes place at the higher critical temperature. The next experiment *appeared* to show, not one recalescence, but several.

Experiment II.—The end of a steel poker was heated to a very bright red in a fire and was then taken to a corner of the room on one side of the fire. The room was quite dark save where it was faintly lighted up by the fire, and the eyes of the observer had been previously rested by shutting them for five minutes before the experiment. The heated part of the poker was intently watched whilst it was cooling and nothing was observed of any note for some little time. Presently, however, the external surface *appeared* to lose in temperature rapidly, and shortly afterwards to brighten most perceptibly. The apparent rapid darkening and reglow occurred no less than *seven* times during this same cooling. The experiment was repeated again and again, not only on the same night but on several nights, and as often as it was repeated *seemed* to show most conclusively that there were several reglows. It was noticed, however, that when the poker was loosely held in the hand the apparent reglows occurred more frequently than when it was fixed; and further that, when the outside of the poker was seen to darken, a slight motion of the poker in any direction caused it to brighten. Evidently convection-currents of air could not account for the phenomenon; for though a current of air might cause a sensible darkening of the external surface by its cooling effect, it could not cause the brightening which seemed to result from the motion. As it was thought that the brightening might be produced by the motion of the iron through the earth's magnetic field, the poker was reheated and fixed in a horizontal position above the poles of a rather powerful electromagnet. As soon as one of the darkenings preliminary to a reglow occurred, the circuit of the battery which actuated the electromagnet was closed and then opened again; but there was no sensible brightening as a result of these operations, either at any point of this particular trial or at any point of any of several subsequent trials which were made. The next consideration was, Was the alternate darkening and reglowing a *subjective* sensation[*]? The poker was reheated, and after having been firmly fixed was watched whilst cooling. As soon as the preliminary darkening appeared, the head of the observer was turned slightly and instantly brought back again. The whole operation of turning the head away and back again certainly did not occupy more than a fraction of a second, and yet the metal shone out quite clearly again. Closing and opening the eyes as quickly as possible had a similar effect. No doubt such an instance of subjective sensation is well known to physiologists, but it has given the author a lesson which he will not readily forget respecting the danger of trusting to mere sensation[†].

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[*] The reheating may take place at too low a temperature to produce a reglow.

Several other attempts were made with bars and wires of iron and steel; and in the case of certain wires there seemed evidence of more than one real reglow. In face, however, of the warning given by the above experiment, the author prefers to leave the point still open until he has completed some calorimetric observations which he has in view.

Experiment III.—A flat iron bar, 60 centim. long, 1·5 centim. broad, and 2 millim. thick, was heated at one end to a very bright red, and observed when cooling in a dark room; a reglow occurred at a very high temperature[‡]. The bar was again heated, and whilst in the fire was bent as in fig. 2; it was then kept in the fire a short time longer, and afterwards removed. At first all the bar from A to E appeared to be of the same temperature; but presently a delicate cloud appeared on the portion BC, which probably might have escaped notice

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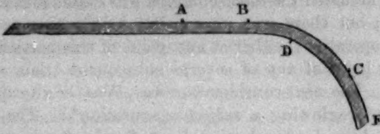
[*] This was suggested by Mr. Burton at the Meeting of the Physical Society at which the paper was read; the author feels much indebted to Mr. Burton for the suggestion.

[†] Possibly the intensity of the apparent reglow will be found to differ with different individuals, and even to vary with the state of health at the time. To the author the phenomenon was very striking.

[‡] Considerably above dull red.

Fig. 2.

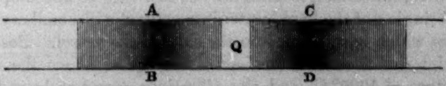


had it not been for the contrast caused by the unclouded portions AB, CE. Very shortly after its formation the cloud rolled away, and immediately afterwards a reglow occurred throughout the whole of the heated portion of the bar. The eyes had been kept upon the convex side of the bar in this trial; and as it was thought that the concave side might possibly show up bright instead of dark, by contrast with the unstrained metal, the bar was heated a second time and again watched on cooling. Rather contrary to expectation, the portion D, when the cooling had reached a certain point, showed up as being slightly clouded; as before, the cloud rolled away and the reglow immediately succeeded, which also, as before, seemed to raise the whole of the heated portion of the bar to the same temperature. The explanation of the formation of the cloud appears to be this: above but near the critical temperature the specific heat of the strained portion is lower than that of the unstrained portion, and, as a consequence, very shortly after the bar has been removed from the fire the temperature of the former is more rapidly lowered than that of the latter. Again, the reason why the strained portion has a less specific heat than the unstrained, and why, after the reglow, the strained and unstrained portions become apparently once more uniform in temperature, may well be that, in consequence of the greater internal friction of the strained portion, or from some other cause, the contraction of its molecules does not go on at so great a rate. When the temperature of a body is raised, part of the thermal energy expended on it is expended in producing *vis viva* of the molecules, and part in pushing them asunder. If we can prevent the molecules from being pushed asunder, a given amount of thermal energy imparted to the metal will produce a greater rise of temperature. Similarly, when the metal is cooling, a given loss of thermal energy will be attended by a greater lowering of temperature when the contraction of the molecules is lessened. As a consequence, when the strained and unstrained portions are cooling near the critical temperature, there is a greater amount of molecular *potential* energy, and therefore a greater rise of temperature when the explosive change occurs at reglow, in the former than in the latter. The next experiment shows that effects similar to the above can be produced by hammering.

Experiment IV.—The straight portion of the bar used in Experiment III. was heated to a white heat, and whilst hot was placed upon a small anvil and struck twice very hard with the sharp end of a hammer at two separate parts, so that two rather deep dents were made in the bar about 10 centim. apart from each other. The bar was now heated a second time to a very bright red, and was allowed afterwards to cool. When a certain temperature had been reached a very beautiful phenomenon presented itself.

Suppose that AB and CD in fig. 3 represent the two dents.

Fig. 3.



Slight clouds were first seen at AB and CD; these quickly deepened and extended both ways, leaving a bright space at Q. Immediately afterwards the clouds rolled away, beginning from Q, and the whole heated portion brightened with the recalescence. This phenomenon was so striking that the heating and cooling were repeated several times, with apparently no very rapid diminution of the effects of the strain. After the bar had been hammered and bent considerably in this and other experiments of like nature, several attempts were made after straightening it to anneal it; these attempts were only partially successful. Always before recalescence faint patches of cloud could be detected here and there, showing that some portions were more strained than others. These clouds do not seem to be formed in those specimens of steel or iron which do not show the phenomenon of recalescence; at least so far as can be judged from observations made with one specimen of steel in which both clouds and perceptible reglow were entirely absent.

Experiment V.—Several attempts were made in this experiment to abolish recalescence by shaking or hammering. For this purpose a piece of pianoforte-steel wire, which showed recalescence well, was selected. The wire, which was 1 millim. thick, was suspended vertically with a weight of 1 kilo. on the end. The weight was held by one hand, whilst the other hand was employed in making the wire vibrate transversely whilst cooling; but no amount of vibrating could prevent the reglow from showing itself. A piece of the same wire was heated to a very bright red in a burner; it was then removed from the burner, immediately placed on an anvil, and a very sharp blow was given it by a hammer. The blow did not prevent the recalescence from appearing.

In this respect, therefore, the behaviour of iron which has cooled below 1000° C. without change of state seems to differ from water which has cooled below 0° C. without freezing; for the latter can be made to freeze by agitation. May not, however, the sudden freezing in this last case be due simply to the bringing of the liquid water into contact with small crystals of ice which have formed on the side of the vessel. Besides, the internal friction of the iron is very great above the temperature of 1000° C., and the vibrations may not have been made sufficiently near the point of recalescence*.

Experiment VI.—This experiment was made with a view of ascertaining whether the reglow would occur at a lower temperature the greater the amount of retentivity of the specimen. A specimen of well-annealed Swedish iron wire showed no trace of reglow. This may have been for two reasons:—In the first place, if the reglow occurs very near the critical temperature the very brightness of the wire renders detection difficult; in the second, the amount of potential energy *suddenly* converted into kinetic energy is less. The author does not believe that in this specimen there was no recalescence, for the following reasons:—Whenever recalescence is very marked the sudden changes which have been mentioned as taking place in a wire cooling whilst under stress and strain are also well marked; and, conversely, if there is no sudden change in a wire under stress or strain there is no recalescence: the recalescence and the sudden changes seem to be inseparable companions. Now, with the particular specimen of iron in question, the sudden changes above alluded to could be detected; but they evidently occurred at a much higher temperature than with those specimens of iron and steel which showed recalescence plainly.

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* It was impossible to continue the agitations right up to the point of recalescence, as the motion prevented the observer from seeing whether the reglow took place or not.

As far as could be ascertained from an examination of about a dozen different specimens of iron and steel, those specimens which seemed most capable of being softened by the process of annealing were those in which the phenomenon of recalescence was least marked. When recalescence is manifest, the temperature at which it appears seems to vary considerably with different specimens: with some this temperature would be at least as high as 800° C., with others at least as low as 550° C.

Views of Professor G. Forbes and Mr. H. F. Newall concerning Recalescence.

According to Professor Forbes[*], the phenomenon of recalescence is due to the fact that at a certain temperature there is a sudden increase of the thermal conductivity of iron. As the metal cools from a white heat, the difference between the temperatures of the outside surface and of the inside will after a time become more or less considerable. The outside, being always cooler than the inside, will sooner reach the critical temperature at which there is a sudden increase of thermal conductivity, and, as a consequence, there will be a sudden rush of heat from the inside to the outside; hence the reglow. The assumption that a sudden increase of thermal conductivity should take place when the metal has cooled to a certain temperature is very reasonable; for there is, without doubt, a sudden increase (or, to speak more correctly, more than one[†] sudden increase) of electrical conductivity as the iron cools.

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[*] Proc. R. S. E. April 6, 1874.

[†] We should expect, therefore, more than one surface-*reheating*.

Mr. H. F. Newall asserts*, however, that he has shown that the reglow is "not due to differences in conductivity in iron at different temperatures," and that "there is a rise of temperature not only at the surface . . . but also throughout the mass." Mr. Newall has not yet brought before us any experimental evidence in proof of these assertions; but he has promised to do so.

Mr. Newall seems to agree with the author that the process going on during recalescence "partakes of the nature of an explosion, in that once started it continues throughout the mass of the iron," but he regards the rise of temperature as being caused by internal *chemical* action. The author looks forward with much interest to the publication of Mr. Newall's results.

XVII. *On the Price of the Factor of Safety in the Materials for Lightning-rods. By* Professor SILVANUS P. THOMPSON, *D.Sc.*†

IT is possible to determine in an absolute manner what metal is best for securing safety by a system of lightning-conductors, for a given prime cost. In the calculations that follow it is assumed that the duration of the lightning-discharge is so brief that there is no time for appreciable radiation or convection of heat from the surface of the conductor, or for conduction into other bodies; and that the conductor is devoid of self-induction.

Safety is dependent, other things being equal, upon the difficulty of fusion of the conductor. The difficulty of fusion of the conductor varies directly as the fusing-point of the material (or, more strictly, as the difference between this temperature and that of the surrounding air, assumed here to be 15° C.), and inversely as the temperature-rise occasioned in it by the discharge of electricity, the amount of which is supposed to be given. The temperature-rise varies inversely as the specific thermal capacity of the material, and directly as the heat developed in it. The heat caused by a given discharge of electricity varies directly as the total electric resistance of the conductor. The total electric resistance of the conductor varies directly as the specific electric resistance per unit cube of volume of the material of the conductor, also directly as its length, and inversely as its area of cross section (supposed uniform throughout). The area of section varies directly as the volume and inversely as the length (supposed given). The volume varies inversely as the density of the material and directly as its mass. The mass of the conductor varies directly as the total cost, and inversely as the cost per unit of mass.

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* Phil. Mag. vol. xxiv. No. 150, p. 436 (1887).

† Read January 28, 1888.

Hence, writing f for the temperature of fusion above that of the surrounding air (assumed at 15° C.), s for the specific thermal capacity, ρ for the specific electric resistance, l for the given length, d for the density, k for the cost, in pence, per pound of the material of the conductor, it at once follows that

$$\text{Safety varies as total cost} \times \frac{f \times s}{\rho \times d \times l^2 \times k}.$$

Hence, as the total cost and the length are supposed to be given, we have

$$\text{Factor of safety for equal total cost} = \frac{f \times s}{\rho \times d \times k}.$$

The only metals about which it is necessary to inquire are copper, silver, iron, lead, platinum, and tin. In the following table the values of f are deduced from the figures of Violle and of Rudberg. The values of k are deduced from the market-prices in London on December 7, 1887: viz. copper, £75 per ton; silver, 4*s.* per troy ounce; iron (rod) £5 10*s.* per ton; lead, £16 10*s.* per ton; platinum, 32*s.* per troy ounce; tin, £170 per ton. The figures given in column 6 for the factor of safety for equal cost are calculated by the foregoing formula multiplied by 10^4. The figures in the seventh column, giving the total cost of equal safety, so far as prime cost of material is concerned, are of course inversely proportional to the figures in column 6.

	f.	s.	ρ.	d.	k.	Factor of safety.	Total cost for equal safety.
Copper ...	1039	0·0949	1615	8·94	8·045	9·488	£100
Silver	939	0·0570	1609	10·51	699·84	0·045	£18,770
Iron	1585	0·1138	9827	7·79	0·589	39·97	£21 9*s.*
Lead	307	0·0314	19847	11·35	1·769	0·242	£3508
Platinum..	1760	0·0325	9158	21·36	5598·7	0·00052	£1,621,600
Tin	213·5	0·0548	13360	7·29	18·22	0·6593	£1287

The prime cost of copper is known to be greater than that of iron for equal conductivity; and reckoning on this basis, irrespective of difficulty of fusion, it is stated in the Report of the Lightning-rod Conference to be 50 per cent. dearer. If that were all, then doubtless the small saving in using iron would be more than counterbalanced by the attendant disadvantages of deterioration by rust, and the like. But when the higher fusion-point, the greater capacity for heat, and the lesser density of iron are taken into account, the superiority of iron, for equal total prime cost, becomes so marked as to once more raise the question whether it may not be expedient to return to the original practice of Franklin, and use lightning-rods of iron instead of those of copper.

London, December 12, 1887.

XVIII. *An Experimental Study on the Influence of Magnetism and Temperature on the Electrical Resistance of Bismuth and its Alloys with Lead and Tin. By* EDMOND VON AUBEL, *of Liége, Member of the Physical Society of London.*[*]

PRELIMINARY COMMUNICATION.

BIBLIOGRAPHY.

1. H. Tomlinson. Proc. Roy. Soc. London, xxxiii. p. 276 (1882).
2. Righi. *Atti della R. Accademia dei Lincei* [3], xix. p. 545 (1883–84); *Journal de Physique* [2], iii. p. 355 (1884).
3. Hurion. *Journal de Physique* [2], iv. p. 171 (1885).
4. Leduc. *Journal de Physique* [2], iii. p. 362 (1884), and v. p. 116 (1886).
5. Von Ettingshausen and Nernst. *Wien. Ber.* xciv. Abth. ii. p. 560 (1886).
6. J. Faé. *Lumière électrique*, xxiii. p. 169 (1887), and xxv. p. 630 (1887).
7. Goldhammer. *Annalen der Physik*, 1887, no. 7, p. 360.
8. Von Ettingshausen. *Sitzungsberichte der Akademie der Wissenschaften zu Wien*, Meeting of the 31st March, 1887.
9. Elie. *Journal de Physique*, September 1887.

MANY physicists have been at work upon the influence of magnetism on the electrical conductivity of metals. The metals which have already been studied are iron, nickel, cobalt, antimony, tellurium, and especially bismuth.

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[*] Read January 28, 1888.

The above investigations have been carried on principally with the view of discovering an explanation of Hall's phenomenon.

The present memoir is intended to fix the date of publication. Our researches were commenced last May; since then many works have appeared on the question which we were studying, notably the excellent memoirs of Messrs. Goldhammer[*], A. von Ettingshausen[†], and W. Nernst[‡].

Quite recently we saw, in the *Wiener Anzeiger*, 1887, p. 222, that Messrs. von Ettingshausen and Nernst had made a communication to the Academy of Sciences at Vienna, having the title "Ueber das thermische und galvanische Verhalten einiger Wismuth-Zinn-Legirungen im magnetischen Felde." This paper is not yet published, as far as we know; so that we cannot take account of it in what follows.

Our researches are far from being finished. They will be continued as soon as our other occupations will permit.

I. *The Preparation of Rods of Bismuth.*

We have studied bismuth under three molecular states :—

(1) Melted and cooled slowly,
(2) Melted and rapidly cooled or tempered,
(3) Compressed.

Let us examine the preparation of rods of bismuth in these three cases.

1. In order to prepare bismuth wires slowly cooled, capillary glass tubes were procured, to which were joined at both ends, at right angles to the capillary part, glass tubes of fairly large diameter. These capillary tubes were heated in a sand-bath, and some grains of bismuth were introduced into one of the side branches. When the metal was melted, it ran down by its own weight into the capillary tube. The

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[*] We have given in the Bibliography only the list of works relating to bismuth.

[†] *Vide* the Bibliography.

[‡] *Annalen der Physik*, 1887, No. 8*b*.

capillary tubes were then left to cool very slowly; they were taken from the sand-bath only when it was quite cold. It often happens that, owing to the unequal expansions produced during the cooling of the bismuth and the glass, the tubes are broken; for this reason we used capillary tubes with very thin walls.

Working in this way, any formation of oxide in the capillary part is entirely avoided; and this part alone must be used in the experiments.

We think that this way of preparing bismuth wires is much more convenient than the method by exhaustion employed by others*, and which cannot furnish results entirely comparable, because there is no certainty that the melted metal is always cooled under the same conditions. The bismuth rod being thus prepared, the two side branches which are not wanted are taken away.

The electrodes to convey the electric current remain to be fixed. For this purpose one end of the capillary rod of bismuth is heated at the same time as a platinum wire in a Bunsen burner up to the melting-point of bismuth; then the red-hot platinum wire is plunged into the capillary tube. It is easy to see that a good contact is thus produced.

2. In order to obtain bismuth rods very rapidly cooled or tempered, the melted metal was poured rapidly into a kind of iron trough, cold, and forming a somewhat acute dihedral angle. If the pouring takes place rapidly, by inclining the trough sufficiently to cause the melted bismuth to run down very quickly, rods of bismuth can be obtained sufficiently long and not too thick. This last condition is indispensable, otherwise the bismuth rods will have too small a resistance.

No adherence takes place during this tempering between the iron and the bismuth. Moreover, by taking certain precautions, the surface of the bismuth may also be filed, and a fresh part of the metal be thus exposed.

In this case the electrodes were made of copper wire, soldered to the two ends of the rod of bismuth by means of a fusible alloy.

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* See notably the memoir of Leduc, *Journal de Physique* [2], iii. p. 363.

3. The compressed bismuth which we used in our experiments was given to us by Prof. Spring. Having been prepared in a drawplate*, it was absolutely cylindrical.

The electrodes were formed, as in the case of the tempered bismuth, of copper wires soldered to the bismuth with a fusible alloy.

The molecular arrangement in this compressed bismuth is very remarkable. A fracture normal to the axis of the cylinder presents a radial structure. We shall see later on the curious results that this sample has given us.

The alloys have only been studied under two states :—

(1) Melted and very slowly cooled,
(2) Tempered.

Rods of these alloys have been prepared in the same way as those of bismuth.

II. *Alloys.*

To obtain the alloys, two sandstone crucibles are used. Bismuth is put into one, tin or lead into the other. These metals have previously been weighed. When the bismuth is melted it is kept liquid, being warmed gently to avoid too much oxidation; it is then poured upon the other metal, which is also liquid. The contents of the crucible are then poured into the other, and this decanting is repeated several times. We have worked in this way in order to obtain alloys in which the proportion of tin or lead should be given almost exactly by the weight, and to avoid having to make analyses of the alloys in order to know their approximate composition.

III. *Analyses of the different Specimens of Bismuth studied.*

We have measured the resistance of several specimens of bismuth. They came from the manufactory of chemical products of Messrs. Monheim at Aix-la-Chapelle, Trommsdorff at Erfurt, and Schering at Berlin. Mr. Monheim's bismuth is commercially pure metal. We have three different specimens from the firm of Trommsdorff, which we shall call, for brevity, "Trommsdorff I.," "Trommsdorff II.," and "absolutely pure Trommsdorff."

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* *Annales de la Société Géologique de Bélgique*, t. xi. p. cxxxiv (1884).

The two first, as well as the bismuth from the Schering factory, are products referred to as very pure in the catalogue of manufacturers.

The "absolutely pure Trommsdorff" bismuth, of which the cost was far higher than that of the others, has been specially prepared for us in the Erfurt manufactory.

As to the compressed bismuth, it has been obtained by means of the commercial metal. Prof. Spring dissolves this metal in HNO_3, precipitates it by pouring the solution into a large quantity of water, filters, and washes it. He then redissolves the basic nitrate, precipitates a second time, calcines, and reduces in a current of pure hydrogen.

We give all these directions, so that it may be possible for physicists to repeat our experiments, and in order to show how little confidence ought to be placed in certain determinations which have been made on the physical constants of bismuth.

M. A. Classen, Professor of Analytical Chemistry at the Upper Technical School at Aix-la-Chapelle, was so kind as to undertake to make qualitative analyses of various specimens for us. We wish again to thank him here for the indispensable assistance which he has given to our work.

Results of the qualitative analyses :—

Bismuth.	Impurities.
Monheim . . .	*Copper**, *lead*, iron.
Trommsdorff I. .	*Iron*, copper.
Trommsdorff II. .	*Lead*, copper, nickel.
Absolutely pure Trommsdorff .	*Lead*, copper (traces), *iron*, carbon.
Schering . . .	Copper, lead ?, nickel.

It is very difficult to prepare pure bismuth. Even the method indicated above for preparing the metal which has been compressed, does not yield a pure product. A small quantity of lead and copper is always precipitated with the bismuth; and it is only by repeating this operation several times that the metal can be obtained very pure.

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* The metals whose names are in italic are those which were found in considerable quantities in the bismuth analysed.

M. A. Classen is occupied at the present time in preparing for us pure bismuth, which is to be used in our later researches.

It results from all this that the various specimens of so-called pure bismuth are very different from a chemical point of view. Their points of fusion and their specific weights have been likewise determined; they vary very sensibly from one sample to another.

IV. *Measurement of Electrical Resistances.*

The threads of bismuth were placed in a water-bath, which was heated all along its length by a linear gas-burner, forming a series of small flames: a thermometer indicated the temperature.

The whole was placed between the poles of a Ruhmkorff's electromagnet of large size, worked by an Otto gas-engine and a dynamo-electric machine of Siemens and Halske*.

The intensity of the electric current in the spirals of the electromagnet was sensibly 28 amperes during the whole course of the experiments.

The ordinary poles of the electromagnet were replaced by large flat poles, made of thick iron circular plates, each one having a thickness of 15 millimetres and a diameter of 150 millimetres. In this way the magnetic field was made more uniform.

In order to measure the resistances, Thomson's method was employed, and a Siemens's *dead-beat* galvanometer, with the magnet in the form of a bell, was used.

In order to avoid heating the thread of bismuth by the passage of the current, one Grove cell only was used, and, by the aid of a see-saw commutator, the current was not allowed to pass in the bismuth for a longer time than was necessary for the observations.

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* The electromagnet was set in such a direction as to have no influence on the galvanometer.

The deviations of the galvanometer-mirror were observed in a telescope furnished with a graduated scale, the distance between each division being two millimetres, and placed at a distance of more than seven metres from the mirror. One can judge of the sensitiveness of the method. We have also employed Kirchhoff's method of measuring resistances, preserving all the other arrangements. A great number of rods of bismuth and its alloys have been tried; in the following tables we only give the values obtained for a sample of each kind.

In our experiments we did not obtain directly the resistance of the thread of bismuth, because in Thomson's method the wires for conveying the current were not fixed directly to this metal, but to the copper or platinum conductors which served as electrodes. It was necessary therefore to deduct the resistances of these copper or platinum conductors, in order to obtain values relating only to the bismuth. In the case of the wires slowly cooled, we have subtracted the values obtained directly, as the resistance of the platinum conductors exterior to the wire of bismuth, because this latter metal adheres very easily to platinum, and the section of the rod of bismuth is very great when compared with that of the thread of platinum.

V. *Results of Electrical Measurements.*

The values of W are the electrical resistances in Siemens's units; the values of W*m* are the electrical resistances in the same units under the action of a magnet.

	Temp.	W.	W*m*.
(a) *Rods of Bismuth slowly cooled.*			
Trommsdorff bismuth, absolutely pure..........	17·2°	0·4100	0·4120
	42·7	0·4050	0·4070
	70·8	0·3953	0·3966
Trommsdorff bismuth I.	20·4	0·1000	0·1010
	42·4	0·1092	0·1099
	69·9	0·1210	0·1217
Monheim bismuth	18·6	1·1295	1·1455
	41·2	1·0425	1·0525
	72·2	0·9375	0·9435
Schering bismuth.........	18·2	0·2522	0·2572
	43·5	0·2652	0·2662
	71	0·2789	0·2789
Trommsdorff bismuth II.	20·4	0·1661	0·1671
	43	0·1713	
	73	0·1811	0·1821
(b) *Rods of Bismuth tempered.*			
Trommsdorff bismuth, absolutely pure..........	15·2	0·1788	0·1790
	40·7	0·1805	
	69·7	0·1812	0·1812
Monheim bismuth	18·4	0·0934	0·0940
	43	0·0932	0·0937
	70	0·0909	0·0911
Trommsdorff bismuth II.	16·2	0·0784	0·0789
	40·3	0·0774	0·0777
	74·2	0·0765	0·0770
Schering bismuth	20·2	0·1239	0·1254
	42	0·1239	0·1249
	72·4	0·1244	0·1249
Trommsdorff bismuth I.	18·2	0·0510	0·0515
	41·6	0·0512	0·0517
	71·4	0·0541	0·0545
(c) *Alloys with Tin.* (1) Rods slowly cooled.			
Absolutely pure Trommsdorff bismuth. 0·39 gr. tin to 100 gr. Bi.	19	0·3364	0·3374
	42·8	0·3444	0·3454
	71·6	0·3475	0·3475
Trommsdorff bismuth I. 0·46 gr. tin to 100 gr. Bi.	19·6	0·3966	0·3976
	45·2	0·4066	0·4066
	70·6	0·4166	0·4166
Schering bismuth. 0·40 gr. tin to 100 gr. Bi.	19·8	0·3643	0·3663
	41·2	0·3733	0·3743
	69·8	0·3803	0·3813
Trommsdorff bismuth II. 0·46 gr. tin to 100 gr. Bi.	21	0·5133	0·5183
	42·6	0·5258	0·5288
	71·8	0·5343	0·5363

Table (*continued*).

	Temp.	W,	W*m*.
(c) *Alloys with Tin* (cont.). (2) Rods tempered.			
Trommsdorff bismuth, absolutely pure. 0·39 gr. tin to 100 gr. Bi.	18·6	0·2133	0·2136
	41·6	0·2173	0·2178
	70·4	0·2213	0·2213
Schering bismuth. 0·40 gr. tin to 100 gr. Bi.	16·6	0·2100	0·2105
	41·8	0·2145	0·2150
	73·9	0·2148	0·2150
Trommsdorff bismuth I. 0·46 gr. tin to 100 gr. Bi.	20·4	0·1443	0·1443
	42·8	0·1478	0·1478
	69·6	0·1498	0·1498
Trommsdorff bismuth II. 0·46 gr. tin to 100 gr. Bi.	24·5	0·1529	0·1534
	40·6	0·1554	0·1561
	77·8	0·1574	0·1574
(d) *Alloys with Lead.* (1) Rods slowly cooled.			
Trommsdorff bismuth, absolutely pure. 0·52 gr. Pb to 100 gr. Bi.	18·8	0·4925	0·4945
	44·7	0·4995	0·5015
	68·9	0·5015	0·5035
Trommsdorff bismuth I. 0·54 gr. Pb to 100 gr. Bi.	21	0·2713	0·2743
	43·4	0·2778	0·2788
	71·6	0·2878	0·2888
Schering bismuth. 0·49 gr. Pb to 100 gr. Bi.	18·8	0·2631	0·2656
	44·8	0·2611	0·2632
	67·4	0·2620	0·2630
Trommsdorff bismuth II. 0·60 gr. Pb to 100 gr. Bi.	15·4	0·3923	0·3963
	43	0·3878	0·3913
	69·8	0·3848	0·3871
(2) Rods tempered.			
Trommsdorff bismuth, absolutely pure. 0·52 gr. Pb to 100 gr. Bi.	18·2	0·1690	0·1700
	42·4	0·1725	0·1730
	70·4	0·1735	0·1735
Trommsdorff bismuth I. 0·54 gr. Pb to 100 gr. Bi.	17·4	0·0937	0·0947
	41·6	0·0929	0·0934
	71	0·0914	0·0919
Schering bismuth. 0·49 gr. Pb to 100 gr. Bi.	18·8	0·1478	0·1493
	44·4	0·1458	0·1468
	70·2	0·1439	0·1449
Trommsdorff bismuth II. 0·60 gr. Pb to 100 gr. Bi.	22·2	0·1056	0·1066
	43·6	0·1056	0·1061
	72·2	0·1031	0·1031

Table (*continued*).

(e) *Compressed Bismuth.* (From Prof. Spring.)

	Temp.	W.	W*m*.
Compressed bismuth ...	16·8	0·1148	0·1157
	42·4	0·1140	0·1145
	76	0·1150	0·1153
The same compressed bismuth, melted, cooled very slowly in a sand-bath, and then operated upon again.	16·2	0·1861	0·1871
	42·3	0·2051	0·2053
	71·7	0·2263	0·2263

VI. *Inferences drawn from the preceding Tables.*

1. *The Influence of Temperature.*—If the preceding results be examined, it is easy to see at once that the different samples of bismuth examined behave quite differently. Some of them give an increased resistance, and others, which is remarkable, a diminished resistance, when the temperature rises. This last fact, noticed for the first time by M. Righi, is not due, according to this physicist, to the presence of arsenic as an impurity in the bismuth, but ought to be attributed to the tin, which, even in very small quantities, is able to produce this result.

A number of experiments have been made by us to discover the reason of this diminution of resistance. We studied first alloys of bismuth and tin, in which the latter metal was present in comparatively large quantities. On examining Table (c), it will be seen that these alloys always give an increase of resistance with the temperature, while the bismuth which enters into their composition produces a diminution of resistance for a rise of temperature. Besides, the chemical analysis has shown us that not one of our samples of bismuth contains any tin as an impurity. It is then sufficiently demonstrated that it is not the metal that is the cause of the observed anomaly[*]. A glance at the results of the chemical analyses and at the electrical measurements will equally prove that the reason for the fact ought not to be looked for in the presence, as impurities, either of arsenic or of iron.

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[*] See also the memoir of Mr. W. Nernst (*Annalen der Physik*, 1887, p. 783), which has appeared during the course of our researches.

Mr. C. W. Kayser[*] has shown that expansion on solidification has not taken place when bismuth is combined with 10 per cent. of lead.

For this reason we have examined also the alloys of bismuth and lead, which have furnished us with very interesting results[†], recorded in Table (d), but which do not in any way explain the diminution of resistance when the temperature rises.

Some influence might still be attributed to the capillary glass tube in which the wire of bismuth is placed. On heating, the glass and bismuth expand differently, and mutually impede each other in their movements. We have therefore successively studied a rod of bismuth cast in a capillary tube, and the same rod after having broken the glass (with precautions) in such a way as not to alter the metallic wire; no change was observed in the direction of these phenomena. Besides, the rods of tempered bismuth were not prepared in glass tubes, and yet some of these present the same peculiarity.

We likewise assured ourselves that there was no relation between the abnormal electrical phenomenon and the points of fusion or specific gravities of the different kinds of bismuth[‡].

2. *Influence of Magnetism.*—On the whole the influence of magnetism on the resistance of bismuth has been more feeble than that which has been announced by M. Righi[§]. Magnetism always produces an increase of resistance; its influence diminishes when the temperature rises, and is more feeble in the alloys than in the bismuth itself.

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[*] *Berichte der deutsch. chem. Gesellsch. Berlin*, 1869, p. 309.

[†] M. Leduc had already studied the conductility of the alloys of lead and bismuth (*Journal de Physique* [2], v. p. 116), but from another point of view.

[‡] Mr. C. L. Weber has made known, in 1886 (*Annalen der Physik* xxvii. p. 145), some very curious results on the influence of temperature on the electrical conductility of the alloys of Rose, Wood, and Lipowitz; but this work has no immediate connexion with our study.

[§] Mr. Goldhammer (*l. c.*) has also found the influence of magnetism more feeble than that indicated by M. Righi.

3. *Study of Compressed Bismuth.*—The resistance of compressed bismuth hardly varies with the temperature; from 16°·8 to 42°·4 there was a slight diminution; then an increase up to 76°, also very small.

After these first experiments, we took away the two ends of the threads of compressed bismuth, on which was the fusible alloy. With the rest we prepared a rod of bismuth slowly cooled by the method indicated above.

Measurements of the electrical resistances of this bismuth, in this state, gave a fairly large increase of resistance when the temperature rose.

There is, then, a notable difference, with regard to the molecular structure, between bismuth prepared in a drawplate and that which, having been melted, has been allowed to cool slowly. One may conceive in this latter case that the crystals take up their own positions during the cooling, whilst the metal prepared in a drawplate is formed of parallel fibres of bismuth.

More complete electrical researches might make clear this important question of molecular physics. One might especially compare bismuth, compressed in an ordinary compressor, with that prepared in a drawplate.

VII. *Conclusions.*

To sum up, the diminution of electrical resistance observed in certain kinds of bismuth and in some alloys with lead cannot as yet be explained. The molecular structure, which we have modified by tempering and compression, has a great influence on the action of the temperature.

We hope that our further researches will lead us to an explanation of the anomalies which we have pointed out.

In the course of this paper we have never mentioned the name of Prof. Ad. Wüllner, our learned and venerated master; we might well have done so at each step. We are glad to be able here to express our thanks to him for the facilities which he has procured for us for undertaking our researches.

Laboratoire de Physique de l'Ecole
Technique Supérieure d'Aix-la-Chapelle.
November 1887.

NOTE.

M. G. P. Grimaldi has recently* studied the influence of magnetism on the thermoelectric properties of bismuth. He denotes by ϵ the thermoelectric electromotive force of the bismuth-copper couple, when it is out of the magnetic field; by ϵ' the electromotive force of the same couple in the magnetic field; and takes for a measure of the phenomenon the quantity $\delta = \frac{\epsilon - \epsilon'}{\epsilon}$.

This physicist finds that δ is positive for commercial bismuth and negative for pure bismuth.

One ought, perhaps, to compare this result with those we have obtained relative to the influence of temperature on the electrical resistance of different kinds of bismuth.—EDM. VON AUBEL.

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* *Journal de Physique*, Dec. 1887, p. 569. This paper is a *résumé* written by the author of a preliminary note presented to the R. Accademia dei Lincei (Feb. 7, 1886) and of a memoir presented in June 1886 to the Societa di Scienza naturali ed economiche di Palermo.

See also *Beiblätter zu den Annalen der Physik*, 1887, no. 6, p. 472.

PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

APRIL 1888.

XIX. *On a Modified Water-dropping Influence-machine.*

By Professor SILVANUS P. THOMPSON, *D.Sc.*[*]

THE ordinary form of water-dropping influence-machine, as devised by Sir William Thomson[†], possesses some inconveniences: it requires a double jet of water and special arrangements for high insulation. A simpler form, requiring but one water-jet and mere silk strings (well paraffined) as insulators, has been found by the author to give far less trouble, and to work well for lecture demonstrations.

From a wooden frame are hung by silk strings three simple metal vessels, the highest and lowest being rigidly connected together with a stiff metal wire. The highest is a small cylinder open at both ends; the lowest is an open pot which receives the water. The intermediate vessel is open at the bottom; and is provided at the top with a funnel, the upper rim of which is soldered inside the lip of the cylinder, and its depth such that its central aperture is about at the middle of the cylinder. An insulated wire, recurved as shown, is carried up clear under the funnel in the middle vessel, and should touch the drops as they fall below the aperture. The water-jet, which must have a fine orifice, is inserted about half-way into the uppermost vessel. A single point of water will suffice to gather a plentiful charge. To watch the process of charging, two gold-leaf electroscopes may be connected respectively to the middle and to the lowest vessels.

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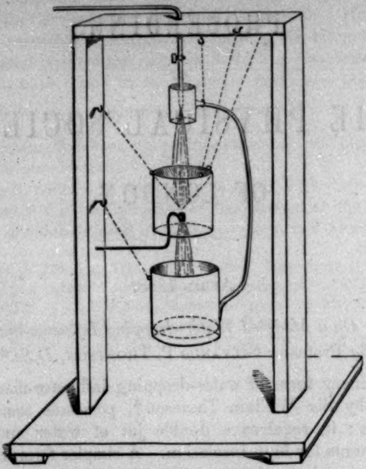
[*] Read January 28, 1888.

[†] Proc. Roy. Soc. June 20, 1867; and Reprint of Papers on Electrostatics, p. 321.



The same arrangement will answer for sand-dropping if a second, uninsulated funnel to contain the sand be provided above the topmost cylinder, and arranged with its lower end entering into the cylinder, so that the jet of sand breaks away from the orifice at the proper height. As dry sand is a very bad conductor, the apparatus is found to work with greater certainty if the sand is previously agitated with finely powdered plumbago, or with some sufficiently adherent metallic powder, such as the finer qualities of Bessemer bronze.

XX. *The Effect of Magnetization on the Thermoelectrical and other Physical Properties of Bismuth.* By HERBERT TOMLINSON, *B.A.*[*]

IN a paper read before the Royal Society on January 26, 1882[†], the author has given an account of an experiment relating to the effect of longitudinal magnetization on the electrical resistance of bismuth. The subject has since been taken up much more fully by Righi[‡], Leduc[§], Hurion[‖], and Albert v. Ettingshausen and Walther Nernst[¶]. In the author's paper quoted above are also described experiments on the effects of magnetization on the electrical resistance of iron, steel, nickel, and cobalt, the results of which are summarized in the following table:—

TABLE I.

Metal.	Condition.	Increase of resistance per unit produced by a C.G.S. unit of magnetizing force, $\frac{\Delta r}{rM_f}$.	Magnetic susceptibility. κ[**].	$\frac{\Delta r}{rM_f} \Big/ \kappa$[**].
Iron	Annealed.	2335×10^{-8}.	30	$0{\cdot}8 \times 10^{-6}$
Steel	Annealed.	1500 „		
Steel	Unannealed.	1137 „		
Steel	Very hard.	70 „		
Nickel	Annealed.	8070 „	8·8	9·2 „
Nickel	Unannealed.	4343 „		
Cobalt. ...	Unannealed.	638 „	4·4	1·4 „
Bismuth ...	Unannealed.	21 „	−·000014[††]	1500 „
Copper ...	Annealed.	0[‡‡]		

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[*] Read January 28, 1888.

[†] "The Influence of Stress and Strain on the Physical Properties of Matter," Phil. Trans. vol. clxxiv. (1883, part 1). Abstract, Proc. Roy. Soc. No. 218 (1882).

[‡] *Acc. R. dei Lincei*, 1883, 1884.

[§] *Bull. de la Soc. française de Phys.* 1884.

[‖] *Comptes Rendus*, 1884, 1885.

[¶] *Sitzb. der kais. Akad. der Wissensch.* 1887.

[**] The values of κ were determined for the same magnetizing forces as those used for producing alteration of resistance.

[††] Taken from Von Ettingshausen's determinations (*Wien. Ber.* 1882).

[‡‡] No change amounting to $\frac{1}{4000000}$ could be detected with a magnetizing force of 90 C.G.S. units.

The data in this table refer to the effects of *temporary* magnetization; and in the case of iron, steel, and nickel, represent only verifications and extensions of the labours of previous observers. Abraham, Edlund, Mousson, and Wartmann all made search for magnetic alteration of the resistance of iron. W. Thomson[*] seems, however, to have been the first to arrive at any definite result. He found the resistance to be increased along the lines of magnetization and decreased across them. W. Thomson has been followed by Beetz[†], Tomlinson[‡], Chwolson[§], Auerbach[‖], and De Lucchi[¶]. These have all confirmed the results of Thomson so far as longitudinal magnetization is concerned; but Beetz failed to obtain anything but negative results with transverse magnetization, and attributed the decrease of resistance observed by Thomson to mere mechanical pull. The author has, however, pointed out[**] the improbability of this last supposition. W. Thomson had also previously proved[††] that the electrical resistance of nickel is increased to a greater extent than that of iron by longitudinal magnetization; whilst Faé[‡‡] has recently verified the author's result concerning cobalt. Lastly, Goldhammer[§§] has published a comparative study of the three paramagnetic metals—iron, cobalt, and nickel, and of the three diamagnetic metals—bismuth, antimony, and tellurium[‖‖].

There are several points in the table given above to which it is desirable to direct attention. In the first place the resistance of all the metals is *increased* by longitudinal magnetization. In the second it by no means always follows that the metals which possess the greatest magnetic susceptibility are those which are most affected in their conductivity by a given amount of magnetizing-stress. We see, for instance, from the third and fourth columns, that whilst the value of $\frac{\Delta r}{r M_f}$ for nickel is twice that for iron, the magnetic susceptibility of iron is between three and four times as great as that of nickel. It is, however, when we come to consider the fifth column that we meet with the most remarkable differences in the effects of magnetization. The numbers in this column represent the increase of resistance per unit which would be produced in each of the metals by magnetizing them to such an extent that each cubic centimetre would possess unit magnetic moment. The effect of the magnetization in this case would be nearly twice as great for cobalt as for iron, twelve times as great for nickel as for iron, and, speaking very roughly, *two thousand* times as great for bismuth as for iron. Startling as this last result is, it sinks into insignificance when contrasted with the results obtained by other observers. The effect of magnetization on the electrical resistance of bismuth is largely influenced, amongst other things, by the amount of impurity in the metal and the magnitude of the magnetizing force. Thus, from Ettingshausen's researches, from which Table II. has been compiled, we learn that the value of $\frac{\Delta r}{r M_f}$ for pure bismuth and for very large magnetizing forces* may become nearly 200 times as great as

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[*] "Electrodynamic Qualities of Metals," Phil. Trans. 1856, Part iv.

[†] Pogg. *Ann.* vol. cxxviii. (1886).

[‡] Proc. Roy. Soc. vol. xxiii. (1875). Also *loc. cit.*

[§] Carl's *Rep.* vol. xiii. (1877).

[‖] Phil. Mag. vol. viii. (1879).

[¶] *Atti del R. Ist. Veneto*, viii. (1882).

[**] *Loc. cit.* pp. 165, 166.

[††] Proc. Roy. Soc. vol. viii. (1857).

[‡‡] *Atti del R. Ist. Veneto*, 1887.

[§§] Wied. *Ann.* xxxi. (1887).

[‖‖] See also a memoir, entitled "An Experimental Study of the Influence of Magnetism and Temperature on the Electrical Resistance of Bismuth and its Alloys with Lead and Tin," by Edmond van Aubel. *Ante*, p. 124.

TABLE II.

Transverse Magnetization.

Magnetizing force, in C.G.S. units.	Increase of resistance per unit produced in pure bismuth by a C.G.S. unit of magnetizing force, $\frac{\Delta r}{r M_f}$.	Ditto in bismuth alloyed with one per cent. of tin.
1600	1610×10^{-8}	
3160	2490 „	
5880	3350 „	
8410	3660 „	660×10^{-8}
10470	3840 „	
11200	3890 „	

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[*] The bismuth was acted upon by a transverse magnetizing force, which, however, has been proved to *increase* the resistance, though to a greater extent than a longitudinally magnetizing force.

that observed by the author[*], with the comparatively low magnetizing force of 130 C.G.S. units; whilst the introduction of one per cent. of tin diminishes the value to *one sixth*. Provided that the susceptibility of bismuth be the same for very high magnetizing forces as for low ones, it would follow that if a bar of pure bismuth could be magnetized to unit intensity[†] its resistance would be nearly trebled, whilst the increase of resistance per unit would be at least 300,000 times as great as the corresponding change of resistance in iron. Considerations such as the above render it difficult to believe that what is observed in bismuth in such experiments as these is a real change in the specific resistance of the metal; and even with iron, nickel, and cobalt there seems to be evidence that the *whole* of the observed change produced by magnetization is not produced by mere rotation of the molecules. In the author's experiments on iron and nickel he found that the increase of resistance could be very closely represented by the formula

$$\frac{\Delta r}{r} = a\mathrm{M}_f + b\mathrm{M} \;;$$

where $\frac{\Delta r}{r}$ denotes the increase of resistance per unit, and M_f and M_i are the magnetizing force and the magnetic intensity respectively. From this formula it follows that, even if the magnetizing force were so high that the ratio of increase of intensity to increase of force was extremely small, the resistance would nevertheless go on increasing very perceptibly indeed with the force. The values of the coefficient b were not very different in the two metals nickel and iron; but the coefficient a in nickel was about five times as great as in iron, and was nearly double the coefficient b. It would be a matter of considerable interest to ascertain whether it would really happen that, when very great magnetizing forces were employed, the resistance of nickel would go on increasing very perceptibly with the force[‡].

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[*] The specimen of bismuth used by the author has been, through the kindness of Professor J. M. Thomson, analyzed at the chemical laboratory at King's College, London; it contains 98·48 per cent. of bismuth.

[†] This would require a magnetizing force of 71,000 C.G.S. units.

[‡] The magnitudes of the forces used by the author never exceeded 200 C.G.S. units.

If, however, on the one hand there is a difficulty in conceiving how magnetization can so largely affect the true specific resistance of bismuth, there is also an equal difficulty in accounting otherwise for what is observed. Hall's phenomenon cannot certainly be credited in any of the experiments which have yet been made with anything but a small fraction of the whole observed effect. It is true that bismuth has been found by Righi and others to have a very large rotational coefficient as compared with iron, nickel, or cobalt; but Ettingshausen and Nernst have shown[*] that whilst, with bismuth, antimony, and tellurium, the increase per unit of resistance produced by a magnetizing force of 7660 C.G.S. units is 0·20, 0·006, and 0·0014 respectively, the corresponding values of the rotatory power are −4·7, +0·18, and +790.

Neither, again, can the change of dimensions produced by magnetization in any of the metals be accountable for the increase of resistance. For though, curiously enough, loading an iron wire increases the resistance, and magnetizing it longitudinally increases the length, whilst loading a nickel wire[†] *decreases* the resistance and magnetization *decreases* the length, yet, according to Joule and others, when an iron wire is loaded to a certain extent longitudinal magnetization begins to decrease the length; whereas the author has shown that, even when iron is loaded nearly to breaking, longitudinal magnetization always produces increase of resistance. Besides, the changes of dimensions in nickel, iron, and bismuth produced by magnetization are far too small[‡]. Here again, however, it should be noticed that both the decrease of length produced by magnetization and the decrease of resistance produced by loading a nickel wire are considerably greater than the corresponding increase in the case of iron.

Whatever it is that causes magnetization to produce so large an effect on the electrical conductivity of bismuth, causes it to produce also a large effect on some of the other physical properties. The thermal conductivity of bismuth is, according to Leduc[§] and Righi[‖], decreased both by longitudinal and transverse magnetization by an amount which is about equal to the amount of decrease produced in the electrical conductivity; and though it would appear, from Ettingshausen's observations*, that the decrease of thermal conductivity is decidedly less than the decrease of electrical conductivity, yet even this observer makes the former comparable with the latter; and we shall now see that the thermoelectrical properties of bismuth are quite as largely affected by magnetization as either the thermal or the electrical conductivity.

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[*] *Loc. cit.*

[†] See Phil. Trans. 1883.

[‡] Prof. Barrett failed to detect any change produced in the dimensions of bismuth by magnetization.

[§] *Comptes Rendus*, 1887.

[‖] *Ibid.*

The Thermoelectrical Properties of Bismuth.

Sir W. Thomson has shown † that iron longitudinally magnetized is negative, and transversely magnetized positive, to iron unmagnetized. Barus and Strouhal ‡ have also investigated with great completeness the influence of magnetization on the thermoelectrical properties of steel of different tempers. Finally, Ewing has entered very fully § into the changes effected by longitudinal magnetization in iron when under different amounts of longitudinal stress. Thomson has also shown ‖ that nickel is rendered by longitudinal magnetization thermoelectrically positive to unmagnetized nickel; whilst the author has found ¶ cobalt when under longitudinal magnetization to be negative to the unmagnetized metal.

The experiment now about to be described was made nearly at the same period as the experiment on the effect of magnetization on the electrical resistance of bismuth and with the same bar. This bar was 25 centim. long and 0·33 centim. in diameter; it was placed in the axis of a magnetizing solenoid, S, specially constructed to avoid imparting heat to the magnetized core**; a preliminary examination proved that there was certainly no error arising from this cause. The arrangements are sufficiently shown in fig. 1, where S is the solenoid and A B the bar. The bar was encircled by two little air-chambers, C and D, through one of which steam was passed, and through the other water at a temperature of about 16° C. The ends A and B were connected by copper wires

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* *Annalen der Physik und Cnemie*, Band xxxiii. (1888).

† "Electrodynamic Qualities of Metals," Phil. Trans. Part iv. 1856.

‡ Bulletin of the U. S. Geological Survey, No. 14 (1885).

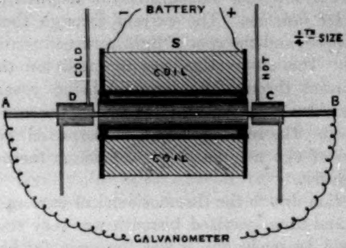
§ Phil. Trans. Part ii. 1886.

‖ *Loc. cit.*

¶ Proc. Roy. Soc. No. 241 (1885).

** For a description of this solenoid see Phil. Trans. 1883, *loc. cit.*

Fig. 1.



with a very sensitive Thomson's reflecting-galvanometer, and were well buried in sawdust.

Since a bar of bismuth can never be obtained in a perfectly homogeneous condition throughout its whole length, there was a considerable thermoelectrical current already in existence before magnetization and the spot of light was driven off the scale. The light could be again brought on the scale by putting the adjusting-magnet low down; but this of course materially diminished the sensibility of the galvanometer, and as the effect to be looked for was likely to be very small this was not desirable. Accordingly the following plan was adopted:—There were two sets of needles in the galvanometer, connected with each other and the mirror; round one set was wound a coil of about 6000 ohms resistance, and round the other a coil of between 7 and 8 ohms resistance; the extremities of these coils were attached to separate terminals, and the latter coil was employed to measure the thermoelectrical effect of magnetization. The thermoelectrical current due to want of homogeneousness in the bismuth was balanced by the current from a Daniell cell sent through the other coil, which was shunted, and through a very large resistance: by altering this resistance the spot of light could easily be brought again to the middle of the scale. Some two hours were allowed to elapse, the steam during the whole of this time passing through the air-chamber, C, and the cold water through the air-chamber, D, after which the spot of light remained steady. The solenoid, S, was now actuated by a current from six Grove cells, and a deflection ensued indicating a current *from unmagnetized to magnetized bismuth through the hot junction.* The current through the solenoid was then stopped, and the spot of light returned sensibly to its old position. The observation was repeated ten times, and then the current through the solenoid being reversed, ten other observations were made, after which the current was again reversed. The readings had to be corrected for a slight direct action of the magnetizing solenoid on the needles of the galvanometer.

The deflection due to the thermoelectrical current between magnetized and unmagnetized bismuth was very small; but, so far as could be made out, it was the same for both directions of the magnetizing current. The mean of the readings gave a deflection of 3·5 divisions of the scale; and, by independent observation with a Daniell cell, it had been ascertained that a deflection of 1 division of the scale would, under the conditions of the experiment, indicate an E.M.F. of 0·143 microvolt. Consequently the E.M.F. produced by temperatures of 100° C. and 16° C. at the two junctions of magnetized and unmagnetized bismuth would be 0·143 × 3·5 microvolts = ½ microvolt. The magnetizing force was 226 C.G.S. units; so that the E.M.F. for unit magnetizing force would be ·0022 microvolt, or ·22 C.G.S. unit of E.M.F. If we divide the last number by 14×10^{-6}, the magnetic susceptibility, we shall obtain the E.M.F. which would be produced by magnetizing the bismuth to unit intensity; this is 15714 C.G.S. units. Now, according to Ewing, the E.M.F. produced in a certain specimen of soft iron wire by a magnetic intensity of 160 C.G.S. units was 6 microvolts, when the junctions of the magnetized and unmagnetized wires were at 100° C. and 16° C. respectively. Accordingly the E.M.F. produced by unit magnetic intensity would be 3·75 C.G.S. From this it is evident that, for a given intensity of magnetization, bismuth has its thermoelectrical properties altered by longitudinal magnetization 4000 times as much as iron. We see, then, that the relative changes produced by magnetization in the thermoelectrical properties of bismuth and iron are comparable with the changes wrought in the electrical and thermal conductivities of these metals.

Grimaldi has already published researches* on the effect of both transverse and longitudinal magnetization on the thermoelectrical properties of bismuth, and finds that magnetization in either of these two directions makes the bismuth of commerce thermoelectrically negative to unmagnetized bismuth. He also finds that the thermoelectrical E.M.F. of a *pure* bismuth and copper couple is increased by both longitudinal and transverse magnetization. Now according to Ettingshausen†, who also quotes Rollmann‡, *pure* bismuth is thermoelectrically *negative* to copper, whilst commercial bismuth is positive to copper. The following table is taken from Ettingshausen's memoir:—

TABLE III.—Bismuth alloyed with different amounts of Tin.

Potential difference, in C.G.S. units, for 1° C. between a bismuth and copper couple with one junction at 20° C. and the other at 0° C.	Number of parts, by weight, of pure bismuth in 100.
−6500	100·00
+ 280	99·05
+1950	98·54
+3910	93·86
+3390	86·90

Consequently it would seem to follow, from Grimaldi's experiments, that *both pure and commercial bismuth* are rendered by magnetization negative to the unmagnetized metal, *i. e.* the thermoelectrical current would flow from unmagnetized bismuth to magnetized bismuth through the hot junction.

Grimaldi shows that, in the following respects, the effect of magnetization on the thermoelectrical properties of bismuth resembles the effect of magnetization on the electrical conductivity:—

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* *R. Acc. dei Lincei*, vol. iii. Also memoir, entitled *Influenza del Magnetismo sulle Proprietà Termoelettriche del Bismuto* (Palermo, 1887); *Journal de Physique*, Dec. 1887, p. 569.

† *Sitzb. der kais. Akad. der Wissensch.* 1887.

‡ Pogg. *Ann.* lxxxiii., lxxxiv., lxxxix.

(1) The amounts of the two effects are comparable with each other.

(2) Transverse magnetization produces a greater effect than longitudinal magnetization.

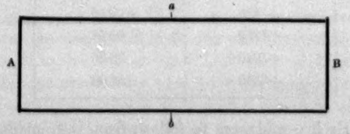
(3) Rise of temperature* diminishes the effect.

(4) The effect increases in greater proportion than the magnetizing force.

It is impossible for the author to compare his own results with those of Grimaldi; but it would seem from the above that, by using high magnetizing forces and lower temperatures at the junctions, the effect of magnetization on the thermo-electrical properties of bismuth might well be found to be some three or four hundred thousand times the effect in iron, upposing both metals to be magnetized to unit intensity.

This being the case, it is difficult to believe that the alteration due to magnetization is a real alteration of the thermo-electrical power of the metal. But, again, how are we to account for it. According to Ettingshausen†, when a plate of bismuth, A B (fig. 2), is arranged, as for experiments on

Fig. 2.



Hall's phenomenon, with its plane parallel to the flat faces of the pole-pieces of an electromagnet and perpendicular to the lines of force, whilst a current of electricity is conducted longitudinally through the plate, the excitation of the electromagnet produces a difference of temperature at the two points *a* and *b*; whilst, on the contrary, if a current of heat be conducted through the plate instead of the electrical current, there will be produced by the action of the electromagnet a difference of potential at the two points* *a* and *b*. Further, besides the difference of potential at *a* and *b*, which is designated the transverse "thermomagnetic effect," there will be a difference of potential at A and B called the longitudinal thermomagnetic effect; and this last, says Ettingshausen, will account for the apparent effect of magnetization on the thermoelectrical properties of bismuth. Both Grimaldi† and Leduc‡, however, are of opinion that the apparent longitudinal thermomagnetic effect is produced by decrease of thermal conductivity and thermoelectrical power.

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* At least as far as 100° C.

† *Annalen der Physik und Chemie* Band xxxi. (1887).

The alteration of dimensions produced by magnetization can as little account for the change in the thermoelectrical properties of metals as for the increase of resistance; for, besides the minuteness of the alteration of dimensions, in some cases the effect of loading on the thermoelectrical power is in the same direction, and in others in the opposite direction, to the effect of longitudinal magnetization, as will be seen from Table IV.

TABLE IV.

Metal.	Under longitudinal traction§.	Under longitudinal magnetization ‖.
Iron	−	−
Nickel	+	+
Cobalt	+	−
Bismuth	+¶	−

There is, however, this resemblance between the effects of magnetization on the thermoelectrical properties of iron and on its dimensions. When an iron wire is loaded beyond a certain limit, magnetization begins to produce decrease of length and increase of thermoelectrical power**.

In conclusion, when we contrast the small magnetic susceptibility of bismuth with the large value of its rotational coefficient, and with the large decrease which can be produced both by transverse and longitudinal magnetization in the thermal conductivity, the electrical conductivity, and the thermoelectrical power of the metal, we must be driven to the conclusion that magnetism in all metals exerts two distinct influences; one by rotation of the molecules about their axes, the other in some way which is not yet understood. In such metals as iron, and to a less extent in cobalt and nickel, the first of these influences probably plays a not unimportant part; but in such metals as bismuth, antimony, and tellurium, the second must entirely predominate.

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* Ettingshausen and Nernst, Wied. *Ann.* xxix. (1886).

† *Nuovo Cimento*, ser. 3, vol. xxii. (1887).

‡ *Comptes Rendus*, 1887.

§ A plus sign shows that the stretched metal is positive to unstretched.

‖ A plus sign shows that the magnetized metal is positive to unmagnetized.

¶ Righi, *R. Acc. dei Lincei, Transunti* (1884).

** *Cf.* Ewing, *loc. cit.*

XXI. *Observations on the Height, Length, and Velocity of Ocean Waves.* *By* Hon. Ralph Abercromby, *F. R. Met. Soc.*[*]

The interest in ocean waves has so much declined in recent years, that physicists have perhaps scarcely realized how much more easily measurements can be taken now than formerly.

In the old days wave-heights could only be ascertained, more or less, by estimation; while the length and speed could only be determined by a common watch. Now-a-days the aneroid can easily measure small vertical heights to within one or two feet; while the fly-back chronograph enables time to be measured to the $\frac{1}{5}$th second, without taking the eye for one moment off the object to be watched.

The following observations were taken on board the S.S. 'Tongariro,' in various parts of the S. Pacific between New Zealand and Cape Horn, in the month of June 1885.

Height was measured by a $4\frac{1}{2}$-inch aneroid with a very open scale, divided to the $\frac{1}{100}$th inch; so that the readings could be taken at a glance to 0·025 inch, or, when time allowed, to 0·020. The instrument is an extremely good and accurate barometer. The altitudes were all calculated on the simple assumption that a difference of 1 foot in height is given by a difference of 0·001 inch of pressure. Any error which could arise between this reduction and that by a more rigorous method would be far less than the other errors of observation.

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[*] Read February 25, 1888.

So far the observation was simple enough; but the great difficulty arose when the height of the eye above the sea-level, at the moment of observation, had to be estimated. For instance, when the barometer was at its lowest point the surface of the water might be 10 feet below the eye; but when the crest of the wave rushed past, the height might be reduced to 1 or 2 feet. Here I had to trust to my own estimation by eye, aided by a few rough measurements with a piece of string down the ship's side. The principal uncertainty of the results depends on errors of this estimation. I do not think that the aneroid errors would ever be more than 2 or 2·5 feet, while those of estimation might be at least 2 feet either way.

Length and velocity were determined by standing in a suitable position with a chronograph, and measuring (1) the interval between the time when two successive crests reached the stern, and (2) the time that the crest of the first wave took to run the length of the ship. Then the length of the ship, her speed, and course relative to the direction of the wave's progress being known, the velocity and length of wave could be readily calculated by the following formulæ :—

Let t = time from crest to crest, in seconds;
T = time of first crest running the length of the ship, in seconds;
l = length of wave, in miles;
v = velocity of wave, in miles per hour;
k = speed of ship, in miles per hour;
L = length of ship, in fractions of a mile;
θ = angle between course of ship and direction of wave-progress.

Then, for a following sea,

$$v = L \cos\theta \frac{3600}{T} + k \cos\theta;$$

$$l = (v - k\cos\theta)\frac{t}{3600}, \text{ in miles},$$

$$= (v - k\cos\theta)\frac{t}{3600} \times 5280, \text{ in feet}.$$

The calculations were made with sufficient accuracy by means of a slide-rule.

It is manifest that it is only possible to obtain an approximation to any of the desired data. Standing near the stern it is not easy to observe the precise moment when the crest reaches the bow; two successive waves rarely run in exactly the same direction; and, with a heavy following sea, the ship yaws about so much that the angle between her course and that of the waves can only be estimated approximatively.

I found great advantage in using cards for the original records, as observations must often be made in rain, sleet, spindrift, and frequently at temperatures near the freezing-point, when the fingers are apt to get benumbed.

The following are some of my most interesting results :—

June 8, 1885, lat. 47° S., long. 175° W.—Sea too irregular to measure individual wave-heights or lengths, but the barometer indicated about 12·5 feet for the vertical motion of the point of observation below decks. Then, pretty constantly, the surface of the sea was about 7 feet below the port-hole in the troughs, but only 1 foot at the crest. This would give the waves an average height of about 18·5 feet.

The velocity of the waves could be got much better. Five observations gave the accordant speeds of 29, 28, 31, 33, and 30 miles an hour, or an average of 30·2 miles.

The following data were used in the calculation :—Ship, length 380 feet; course S.E.; Speed 14 knots. Sea running to S.S.E., and therefore following.

June 10, 1885. Lat. 51° S., long. 160° W. Height.—I found it impossible single-handed to estimate the height of the water at the moment the aneroid was read, so I took a constant difference of 6 feet for the difference of the height of the eye at trough and crest. The following is an example of the readings :—

Trough.	Crest.	Difference.	Diff. height eye, trough and crest.	Estimated h ight, in feet.
28·955	28·975	·020	6	26
·97	·985	·015	6	21
·9675	·985	·0175	6	23·5
·960	·980	·020	6	26

These waves were not consecutive, and the difference between the extreme heights is a good deal larger than the height of any single undulation. The highest reading of the series (not given here) was 28·99, the lowest 28·955, which would give a difference in height of 35 feet.

The length and velocity were measured on deck just before the heights, under the following conditions:—

Ship: length 380 feet, course E.S.E., speed 14 knots. Sea running to S.E., and therefore following.

The following are some of the best observations:—

Time of crest, running length.	Time between two crests.	Velocity. Calculated.	Length. Calculated.
12·5″	19″	32 miles.	507
11·0	15	35 „	470
9·0	...	39·5 „	
16·0	17	28·5 „	358

This sea would have been logged as 6 or 7 on the ordinary scale of 0–8. The wind was blowing a moderate to hard gale from N.W. with heavy squalls, and was logged 7 on Beaufort's scale of 0–12. During some of the squalls, with thunder and lightning, the force rose to 8; and though the ship was never nearly pooped, we managed to split a topsail. I think this might be taken as a fair average sea in the S. Pacific. The waves were far too irregular to allow of any attempt being made to determine the ratio of height to length or velocity.

July 16, 1885. Lat. 55° S., long. 105° W.

Trough.	Crest.	Difference.	Diff. of height eye.	Estimated height.
29·38	29·4025	·0225	6	28·5 feet.
·38	·42	·040	6	46 „
·3775	·40	·0225	6	28·5 „
·38	·41	·030	6	36 „

The readings in the trough belonging to this series are remarkably uniform.

Length and velocity were measured from deck under the following conditions :—Ship, length 380 feet ; speed, 14 knots; course to E. ; sea from W., and therefore right aft.

Time of crest, running length.	Time between two crests.	Velocity. Calculated.	Length. Calculated.
12"·5	14"·25	35·5	445
12·5	16·5	35·5	485
8·0	16·0	47·5	765

This was the heaviest sea we encountered during the whole voyage. The wind was blowing a hard gale from S.W., with squalls of hail and sleet. The sea was certainly high, but did not appear excessively so to the eye. The ship broached to and lay in the trough of the sea for nearly half an hour while a gland in the engine-room was being packed ; but no harm was done beyond splitting a topsail. The ship rolled tremendously, but no seas swept over her.

Under these circumstances the height of 46 feet, given by one set of observations, seems excessive. The actual vertical lift of the cabin was undoubtedly 40 feet ; for, as I noticed it at the time, there is not a mistake in the records. Any error in the true height must come from the estimate of 6 feet for the difference of the height of the eye on the crest and in the trough.

It will be noticed that the relation of length and velocity to height is very irregular ; but this is due to the character of the waves, and not to errors of observation. On all the days the waves were running irregularly. We never saw crests nearly a mile long chasing one another with a well-defined trough between them ; but the seas were so confused, that sometimes after one big crest nothing followed but some small waves. There was nothing to call a cross sea ; but there were many series of waves of different lengths running pretty much in the same direction, which were constantly interfering with one another. An eye estimate of height is always delusive, but I was surprised that the waves measured as much as they did. I have seen a heavier sea in the Atlantic; so that if we take only 40 feet as the highest of this series, it is perfectly certain that much greater heights are sometimes attained.

Fitzroy says that, even during many years spent at sea, a man only observes really high waves once or twice during a lifetime; and this view is confirmed by every sailor I have conversed with. Taking my own measurements as representing only ordinary seas, I am certain that 60 feet at least from trough to crest must be attained by exceptional waves.

It may be interesting to compare the results given here with those obtained by other observers. The following figures, except those in the last line, are taken from Dr. Krummel's *Ozeanographie*.

Table of the Maxima Dimensions of Waves.

Authority.	Locality.	Height, in feet.	Velocity, m.p.h.	Length, in feet.	Period, in seconds.
Lieut. Pâris	Indian Ocean.	......	33·6	374	7·5
Admiral Molter...	....	......	80·0	2703	23·0
Captain Ross * ...	Near C. Good Hope.	22·97	90·0	1902	
Captain Chüden..	33° S., 107° W.	33–36	......	984–1312	
D'Urville	....	98·0			
Scoresby............	....	42·0			
'Novara'	....	36·0			
'Challenger'	....	23·0			
Abercromby	S. Pacific.	46·0	47·5	765	16·5

It is manifest, from an inspection of the above table, that the discrepancies are enormous. I cannot but think that the extreme lengths recorded by Admiral Molter and Captain Ross must have not allowed for the interference of following waves. No doubt such lengths might be observed between two notable crests, but most probably there would be some smaller undulation between. I never took the time between two crests unless they manifestly belonged to a simple wave.

The three sets of observations before noticed were taken on the only three days on which at all big waves were met with. They serve to show the difficulties of wave-measurement; but the greatest obstacle is the uncertainty of meeting first-class specimens. If ever a wave-measuring party should be organized, I think it should be arranged as follows.

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* Dr. Krummel's figures do not agree with those given in other places for Capt. Ross's results.

Three observers, A, B, and C, would be necessary. A would command the party, say when the instruments were to be observed, note personally the height of the deck from the water, and enter all the readings on a suitable card. B would have a suitable aneroid, and confine his whole attention to that instrument. C would be furnished with two chronographs.

When a crest touched the stern of the ship, A would give a signal, on which B would read his aneroid, while C started both his chronographs. A would first note the height of the deck by marks on the ship's side; and, if need be, read a simple clinometer to allow for the roll of the ship before entering the records of his assistants. When the crest reached the bow, A would give another signal for C to stop one chronograph. In the trough, B would read his aneroid, while A noted the height of the water; and, finally, as the next crest came on, B would read his aneroid, C would stop his second chronograph, while A noted the height of the water and entered all the records.

By this means, and with a careful selection of tolerably undeformed waves, I think that the measurements of undulations could be much more satisfactorily obtained than heretofore; and I only regret that the means at my disposal did not enable me to do more towards this important line of research.

SUMMARY.

The results of this paper may be summarized as follows:—

Several sets of observations between New Zealand and Cape Horn, with an aneroid barometer and chronograph, gave for the largest waves a height of 46 feet, a length of 765 feet, a speed of 47 miles an hour, and a time-period of 16·5 seconds.

As nothing but the ordinary heavy weather of these latitudes was experienced, it is certain that waves must sometimes attain a height of at least 60 feet.

Really big seas are of very rare occurrence.

The great discrepancies in the observed elements of waves given by different observers is doubtless due to the varying lengths of every series of undulations, which therefore always make a more or less confused sea.

XXII. *Experiments on Electrolysis.*—Part I. *Change of Density of the Electrolyte at the Electrodes.* *By* W. W. Haldane Gee, *B.Sc., Assistant Lecturer in Physics, and* H. Holden, *B.Sc., Bishop Berkeley Fellow in Physics, of the Owens College, Manchester*[*].

Whilst studying some electrolytic polarization phenomena with palladium electrodes in dilute pure sulphuric acid, a liquid was seen, *after a reversal of the current*, to flow downwards in streaks from the anode. Not being able to find any reference to the formation of streaks, for whose appearance the reversal of the current was necessary, it was decided to investigate their character. Further, it was thought that the occluded hydrogen might, on reversal of the current, unite with the nascent ion liberated at the anode, and thus effect chemical changes of an interesting character.

Some little care in observation and adjustment of the light is necessary in order to see the streaks, which, like the surrounding electrolyte, are colourless, and are only visible on account of the difference between their index of refraction and that of the main bulk of the electrolyte. A vessel with parallel sides of good plate-glass was employed, and a mirror used to reflect light obliquely into the cell. The arrangement which has been found to be most convenient is shown in the figure, and consists of a cell made of two pieces of plate-glass about 15 centim. square. A piece of india-rubber, 25 centim. long and 3 centim. square section, is bent in a semicircular form and clamped between the two glass plates by means of four iron screw-clamps. In this way a water-tight cell, about 1·5 centim. broad, is obtained which may readily be taken to pieces for the purpose of cleaning. The electrodes have usually consisted of two pieces of palladium, about ·05 centim. thick and with a surface of ·5 square centim., fastened to platinum wires and supported by means of a convenient electrode-holder.

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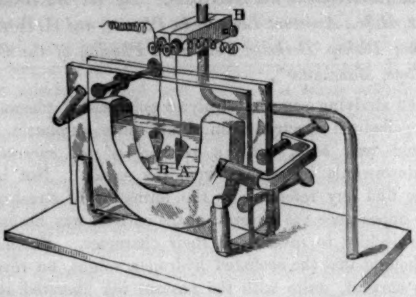
[*] Read February 25, 1888.

This is the first of a series of papers on Electrolysis and Electrolytic Polarization, descriptive of experiments made, during last year, at the Owens College Physical Laboratory. An abstract of the experiments made, to the end of August 1887, was submited to the British Association Meeting at Manchester. We desire to acknowledge the assistance received up to that time from Mr. C. H. Lees, B.Sc., Derby Mathematical Scholar of the Owens College. His cooperation has since been discontinued, owing to absence at Strasburg.



Streaks obtained in dilute pure Sulphuric Acid.

The electrodes were first heated to redness in order to drive out any occluded gas, and then cleaned by means of glass-paper. They were then placed, by means of the electrode-holder H, in the electrolytic cell containing dilute pure sulphuric acid. The current was sent from one electrode (A) to the other (B), which is thus the kathode, for a certain time and then reversed. On reversal no gas appears at first from B, which is now the anode, but streaks resembling a dense liquid are with careful observation seen flowing downwards from this electrode. After a time, depending on the size of the electrodes, the strength of the current, and its duration in the first direction, evolution of gas at B begins, and the streaks simultaneously cease to be visible. The streaks are to be seen streaming from one electrode only at a time, and always descend from that palladium electrode which first serves as kathode and then as anode.

When platinum electrodes are substituted for the palladium ones, we have not been successful in obtaining the streaks; it is, however, only necessary that one of the electrodes should be palladium in order to obtain the streaks.

An experiment was made in order to ascertain the part which the reversal of the current plays in the formation of the streaks. The current was sent in one direction through the electrolyte, the palladium electrodes then taken out, washed, and their surface cleaned with glass-paper. On now reversing the current and replacing the electrodes, the streaks are quite as evident as if the electrodes had not been taken out. But if, on taking the electrodes out, they are heated to redness instead of merely having their surface cleaned, on replacing them and reversing the current the streaks are not seen, and gas appears immediately from the anode. We concluded from this experiment that the function of the current in the first direction was to fill with hydrogen that electrode at which the streaks appear in the second direction of the current; and that the reason why, after a time, the streaks ceased to be visible, and simultaneously gas began to be evolved at the anode, was that by this time all the occluded hydrogen had been used up. It was therefore thought probable that the streaks were formed by a combination of the occluded hydrogen with the ion (either SO_4 or some of its components, such as oxygen) which is liberated at the anode in the second direction of the current. The theory that the streaks were composed of concentrated sulphuric acid seemed open to great objections; for we know that concentration takes place at the anode in dilute sulphuric acid without a reversal of the current, and also does not depend on the use of palladium electrodes; but the streaks are, as is shown above, only to be obtained after a reversal of the current, and are not to be obtained with platinum electrodes. It was therefore decided to test, as far as practicable, other possible combinations, such as hydroxyl (the streaks cannot be composed of water, for they are denser than the surrounding electrolyte), a salt of palladium, &c.

Testing for Hydroxyl.

The palladium electrodes used were rectangular strips, about 2 centim. long and ·3 centim. broad, placed vertically in 10-per-cent. sulphuric acid. For the purpose of collecting the streaks a capillary glass tube had one end blown out into the shape of a small funnel, and was bent so that it could be lowered into the electrolyte with the finger pressed on the upper end, which projected from the cell, until the funnel part was immediately below the electrode. After a reversal of the current the funnel part was placed in this way under the anode, from which the streaks were flowing. The finger was then removed from the top end of the tube, and the funnel acted as a reservoir for the denser liquid flowing from the anode. As soon as gas appeared at the anode the finger was replaced on the top end of the tube, the latter being then removed and its contents emptied into a test-tube. This proceeding was repeated many times (at least fifty), and the total liquid collected was tested for hydroxyl by the addition of a few drops of a solution of titanic acid in strong sulphuric acid. No trace of a coloration could be perceived, and thus the presence of hydroxyl was not proved[*].

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[*] The test, which is a delicate one, has been greatly employed by Richarz in his experiments on the mode of appearance of hydroxyl at the anode in sulphuric acid (Wied. *Ann.* xxxi. p. 912, 1887). He concludes that the appearance of hydroxyl at the anode is caused by the purely chemical decomposition of the "Ueberschwefelsäure" (Berthelot's $S_2O_7+H_2O$), which is first formed there. Traube, *Berichte der deut. chem. Gesell.* xviii. p. 3348 (1888) gives the equation for this decomposition as

$$S_2O_6(O_2)H_2+2\,H_2O=2H_2SO_4+H_2(O_2).$$

Richarz confirms Berthelot's statement that no hydroxyl is formed at the platinum anode unless the strength of the acid is above 60 per cent. As we obtained the streaks in 10-per-cent. acid, the above fact lends additional strength to the view that the streaks are not hydroxyl. They might consist of $H_2S_2O_8$ (Ueberschwefelsäure), which Berthelot has shown to be a minor secondary product formed during the electrolysis of sulphuric acid, whether dilute or strong (see *Ann. de Chim. et de Phys.* xiv. 1878, and xxi. 1880). The quantity so formed at a platinum anode is small, and would apparently be still smaller at a palladium anode charged with hydrogen if the following reasoning be accepted. Let us first consider the case of an anode uncharged with hydrogen. Assuming that the ion liberated at the anode is SO_4, the simplest hypothesis to account for the secondary reactions is that part of the SO_4 combines directly with the H_2SO_4 of the electrolyte to form $H_2S_2O_8$, whilst the remainder of the SO_4 (probably by far the greater part) acts on the H_2O of the electrolyte, forming H_2SO_4 and liberating O. Should, however, the anode be charged with H, the SO_4 would have an additional tendency to combine directly with the H, forming H_2SO_4. This tendency would probably greatly

Testing for Palladium Salts.

A supply of the liquid having been obtained by a similar process to the above, a solution of potassium iodide was added to it, but no evidence of the presence of palladium was obtained.

Streaks obtained in Phosphoric Acid.

Similar streaks were obtained in solutions of pure orthophosphoric acid by adopting exactly the same procedure as with sulphuric acid.

Streaks obtained in Caustic-Soda Solution.

In this case also streaks were obtained descending from one of the electrodes, but the conditions necessary for their appearance are very different to those in the case of sulphuric and phosphoric acids. *No previous reversal of the current is necessary*, and the streaks descend from the kathode immediately the current is passed. The gas (hydrogen) formed at the kathode is at first absorbed, and the streaks are seen to descend as long as the absorption takes place; but, when gas begins to be evolved briskly, the streaks disappear. By very careful observation it is seen that the liquid of which the streaks are composed is still formed, but is carried up in the current of gas to the surface of the electrolyte, from which it rebounds, giving the electrolyte round the kathode the peculiar wavy appearance which accompanies the incomplete mixing of two liquids of different densities, such as two strengths of the same solution.

Explanation of the Caustic-Soda Streaks.

This experiment led us to infer that the streaks in the caustic-soda solution are composed of concentrated alkali, which is known to be formed at the kathode, and that the

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exceed the other two, so that if sufficient occluded H be present, it would be expected that neither O would be liberated, nor $H_2S_2O_8$ formed. It is known that there is no evolution of O under these conditions; and it would be interesting to estimate the relative amounts of $H_2S_2O_8$ produced at a palladium anode when charged and when uncharged with H. The $H_2S_2O_8$ could be quantitatively determined by the method used by Richarz (*loc. cit.* p. 917).

absence of evolution of gas from that electrode is a necessary condition for their appearance and steady downward flow. The part played by the palladium in the formation of the streaks is, according to this theory, that of absorbing the hydrogen, which otherwise, in escaping from the electrode, would carry the concentrated solution along with it to the surface and thus prevent the formation of the streaks.

Explanation of Streaks in general.

Since in acids concentration occurs at the anode, the descending streaks, if seen at all, should be seen at that electrode. But taking sulphuric acid as an instance, oxygen is given off at the anode, and the palladium cannot retain it sufficiently to stop its evolution, and thus the concentrated acid will be carried up by the current of oxygen and escape observation. But if the electrode has been filled with hydrogen by previously serving as kathode, the evolution of oxygen on reversal of the current is prevented either by its direct union with the occluded hydrogen, or by the union of the ion SO_4 with the hydrogen. No gas being evolved, the concentrated acid is able to flow downwards in streaks. This explanation is borne out by the experiments with sulphuric acid detailed above, in which it was seen that, by placing freshly-heated palladium electrodes in sulphuric acid, gas is immediately evolved at the anode; but if that electrode has been previously filled with hydrogen, the oxygen, on reversal of the current, does not appear for some time at the anode, and the streaks are visible during this period.

Concentration of the solution at one electrode is accompanied by a weakening of the solution at the other electrode. Therefore, as in acids the weakening takes place at the kathode (at which the gas is absorbed), we should expect to see streaks ascending from that electrode without a previous reversal of the current. On trying the experiment with palladium electrodes placed *horizontally* in dilute sulphuric acid, this supposition is found to be warranted. The reason why the streaks ascending from the kathode were not seen in our earlier experiments is, that the electrodes had been placed vertically with their top edges a little below the surface of the electrolyte, and so the weakened solution in ascending had naturally clung to the surface of the electrode and thus escaped detection.

The late Professor Christiani (in a work* to which we shall refer more fully in a subsequent paper) gives three instances in which he observes streaks from the electrodes, but does not offer any explanation of their mode of formation. He cites the cases of zinc electrodes in concentrated zinc sulphate and copper electrodes in concentrated copper sulphate†. In these cases, without a previous reversal, he observed streaks descending from the anode and ascending from the kathode at the same time. These results are evidently in accordance with the theory proposed above: no gas is given off at either electrode, and so the concentrated solution is allowed to descend in streaks from the anode and ascend in streaks from the (horizontal) kathode.

Another confirmation is given by the behaviour of potassium sulphate during electrolysis. After a current has been passed in the same direction through potassium sulphate, it is found, on testing with litmus paper or by cautiously adding litmus solution, that the bottom strata of liquid have become alkaline and the top strata acid, the solution being originally neutral. It follows from this that the alkaline solution produced at the kathode is denser than the electrolyte, which itself is denser than the acid solution produced at the anode. Therefore, according to theory, the streaks, if seen at all, should descend from the kathode and ascend from the anode. This is fully borne out by experiment. Starting with freshly-heated palladium electrodes, on putting on the current we immediately see the streaks descending from the kathode; and after a reversal of the current other streaks may simultaneously be detected rising from the anode.

The best conditions for seeing the streaks seem to be:—

(1) a horizontal electrode of small surface‡,

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* "Ueber irreciproke Leitung electrischer Ströme," 1876, R. Friedänder; Wied. *Elect.* ii. p. 727.

† Christiani, *loc. cit.* p. 100.

‡ A vertical electrode may be used with advantage if it is well below the surface of the electrolyte and if it is suspended by a horizontal wire, so that the view of the electrolyte above and below it is not interrupted. An angular electrode is very convenient, as the liquid flows most readily from points.

(2) a strong current,

(3) no evolution of gas.

The conditions (2) and (3) are incompatible with the use of platinum electrodes in dilute sulphuric acid: it is quite possible, however, that even in this case matters might be so adjusted that some trace of the formation of the streaks could be detected. In the case of platinum electrodes in solutions of sulphates of metals which do not act readily on water, such as zinc, copper, iron, &c., the metal will be deposited and no gas evolved at the kathode, and thus streaks of the weakened solution can be seen at that electrode*.

A direct proof that the streaks were formed of concentrated acid might be thought possible, in the case of sulphuric acid, on consideration of the following circumstances:—

(1) When strong sulphuric acid is dropped into dilute sulphuric acid, the strong acid sinks to the bottom of the containing vessel and remains undiffused for a considerable period.

(2) If collecting-vessels are placed under the electrodes and an arrangement fitted up by which the current may be periodically reversed, say every minute, the streaks will be formed at the electrodes and will stream into the collecting-vessels.

(3) If we now titrate with standard caustic-soda solution equal volumes of the liquid in the collecting-vessels and of the main bulk of the electrolyte, we ought to find that the former is the more acid.

It may, however, be noticed that, assuming the streaks were not concentrated acid, we should collect by this arrangement, not only the streaks, but also some of the concentrated acid which is formed at the anode. Thus, even if the acid in the collecting-vessels did become stronger, it would scarcely prove that the streaks were the cause of this†.

From the experiments detailed above, though direct proofs are apparently not available, we may draw the following general conclusions:—That when gas is not evolved at an electrode, streaks are formed there. These are due either to a concentration or weakening of the electrolyte, as in the case of solutions of acids and alkalies and some salts (such as zinc sulphate); or, in the case of other salts (such as potassium sulphate), to a chemical change in the electrolyte, yielding at one pole alkali and at the other pole acid, producing solutions of different density to the electrolyte. When gas is not evolved at an electrode these changes are still produced, but their effect in producing streaks is destroyed by the evolution of the gas.

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* Christiani noted the streaks with platinum electrodes in ferrous sulphate.

† Some observations have been made by this method, but the results have not been very definite. We have designed an automatic commutator which will reverse the current at intervals of 15, 30, or 60 seconds. This arrangement will enable us to collect more easily the substance forming the streaks.

In our next paper we hope to describe some experiments in which these effects become of great importance in changing the resistance of the electrolyte.

XXIII. *On a Method of Determining the Difference between the Phase of two Harmonic Currents of Electricity having the same Period.* *By* THOMAS H. BLAKESLEY, *M.A.*[*]

IT has been brought to my notice by both English and Foreign journals connected with science that a method of determining the difference in phase of two Harmonic Currents of Electricity having the same period, which I invented and published some years ago, forms an important part of the subject matter of a paper communicated to the Royal Academy of Sciences of Turin, second series, vol. xxxviii., by Signor Galileo Ferraris, in which that philosopher lays claim to the invention above mentioned, producing it as original, with no sign of acknowledgment that the method has before been made public.

The method consists in employing the two coils of an electric dynamometer in a peculiar way. When an harmonic current is sent through the coils of such a dynamometer in series, its reading will measure the quantity $\frac{I^2}{2}$, where I is the maximum value of such a current. In this way we may successively determine $\frac{I_1^2}{2}$ and $\frac{I_2^2}{2}$, where the subscripts refer to two currents having the same period.

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[*] Read March 10, 1888.

But if we place one of the coils in one circuit and the other in the second circuit, the reading of the instrument will measure $\frac{I_1 I_2}{2}\cos\theta$, where θ is the angle representing the phase-difference of the two currents, to which the name "*décalage*" has, I have no doubt with great propriety, been accorded by M. Hospitalier.

It is clear that from the three readings we can deduce the angle of phase-difference. Thus, α_1, α_2, α_3 being the three readings, we have:—

$$\alpha_1 \propto \frac{I_1^2}{2},$$

$$\alpha_2 \propto \frac{I_2^2}{2},$$

$$\alpha_3 \propto \frac{I_1 I_2}{2}\cos\theta;$$

$$\therefore \cos^2\theta = \frac{\alpha_3^2}{\alpha_1\alpha_2}.$$

Here I have supposed that the same instrument is used successively; and it is hardly necessary to point out that if we have three exactly similar dynamometers, or if we have three dynamometers of which the relative values of the constants are known, we can deduce θ by means of three simultaneous observations.

The real importance of the determination of the *décalage* rests in the means it affords us of determining the causes of its existence; among which I may here mention Coefficients of Induction (self or mutual), Capacity of Condensers, and hysteresis or the waste of energy involved in the reversal of electromagnetic momentum, all of which present useful fields for inquiry capable of investigation by means of this application of the dynamometer, a few of which I have myself traversed.

I wish to point out that this method and its importance (not yet fully appreciated) were published by me in the 'Electrician' newspaper of October 2, 1885; and the entire series of papers, of which this formed one, was republished in book form at the end of the same year. This was before I had the honour of membership in the Physical Society, or doubtless I should have then brought the subject more immediately under your notice; but upon my being elected a member I lost no time in presenting a copy to this Society, and those who care to examine this question will find therein a chapter devoted to this very employment of the dynamometer to determine Coefficients of Self- and Mutual-Induction and the Capacity of Condensers.

March 1888.

XXIV. *On the Numerical Relation between the Index of Refraction and the Wave-length within a Refractive Medium, and on the Limit of Refraction.* *By* T. Pelham Dale, *M.A.*[*]

The following equation (see Sir Geo. Airy's Treatise on the Undulatory Theory, ed. 1877, p. 93) is proposed as expressing the relation between wave-length and velocity of propagation in an isotropic medium :—

$$v=\sqrt{\left(1-\frac{1}{2^{\frac{3}{2}}}\right)\frac{m}{}}\cdot\frac{\sin\frac{\pi h}{\lambda}}{\frac{\pi h}{\lambda}}\;.\;.\;.\;.\;.\;.\quad(\alpha)$$

v is here the velocity of propagation, h the distance between the undulating particles, λ the wave-length, m the absolute force of attraction, supposed to follow the law of the inverse square.

For our purpose this may be put under the form

$$v=k\sin\frac{\pi h}{l}\Big/\frac{\pi h}{l}\;.\;.\;.\;.\;.\;.\;.\quad(\beta)$$

It is convenient to reserve λ to signify the wave-length in free æther, and to use l for the wave-length within the medium. The values of the wave-lengths in free æther are those given in the text-book of Glazebrook and the tables of Lupton.

Now, knowing the index of refraction for any fixed line by observation of the index μ, the corresponding value of l is given by the equation

$$l=\frac{\lambda}{\mu}.$$

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[*] Read February 11, 1888.

A convenient notation is l_A, λ_A, μ_A, &c., for index and wave-lengths of the line A &c.

Now if V be the velocity of propagation in free æther,

$$v = \frac{V}{\mu},$$

whence from (β) we have

$$\frac{\pi h v}{l} = k \sin \frac{\pi h}{l}.$$

Calling $\frac{\pi h}{l}$, θ, we have

$$\frac{\pi h v}{l} = k \sin \theta;$$

for any other value l_1 corresponding to index μ_1 we have

$$\frac{\pi h v}{l_1} = k \sin \theta_1,$$

and

$$\frac{\pi h}{l_1} = \theta_1;$$

or

$$\theta_1 = \frac{l}{l_1} \theta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\gamma)$$

Hence if we can find a value of θ for any one wave-length and index, all the others can be found from the observed wave-lengths and corresponding indices.

The angle θ can be readily found, if we know two indices, one corresponding to any given wave-length, and the other corresponding to double the wave-length, both *within* the medium.

For $\frac{\pi h}{l} = \theta$,

then

$$\frac{\pi h}{2l} = \frac{\theta}{2},$$

$$\frac{\pi h v}{l} = k \sin \theta = 2k \sin \frac{\theta}{2} \cos \frac{\theta}{2},$$

$$\frac{2 \pi h v'}{2l} = 2k \sin \frac{\theta}{2};$$

$$\therefore \frac{v}{v'} = \cos \frac{\theta}{2},$$

or

$$\frac{\mu_{2l}}{\mu_l} = \cos \frac{\theta}{2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\delta)$$

Now μ and μ_1 can be found either by observation or interpolation. If H be the line chosen, then, practically, the index of a wave-length twice as long in the medium will not be far from A; this we may call μ_{2H} and speak of it as the index of the octave below.

It is evident that if k remain constant, $\frac{k \sin \theta}{\theta}$ continually approaches k as θ diminishes. Now if l be increased and h and m remain constant, θ diminishes; hence there must be a limit of refraction in the case of waves of very great length. This limit, denoted by ν, is evidently found by the equation

$$\nu = \frac{\mu \sin \theta}{\theta}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\epsilon)$$

This is in effect the limit found by Cauchy in a somewhat modified form.

Since
$$\nu = \mu \frac{\sin \theta}{\theta},$$
$$= \mu_1 \frac{\sin \theta'}{\theta'},$$

it follows on substitution of $\frac{\pi h}{l}$ for θ and $\frac{\lambda}{\mu}$ for l, and dividing out, that

$$\sin \theta = \frac{\lambda}{\lambda_1} \sin \theta_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad (\zeta)$$

Combining this with the equation above,

$$\theta = \frac{l_1}{l} \theta_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\eta)$$

we can find a value of θ which satisfies both these equations from any two values of wave-length and corresponding index. And from this value of θ the corresponding values of θ to any other index and wave-length can be found.

In practice this equation can be solved by trial without much trouble, especially when a large number of indices of substances of similar refractive power have to be investigated, when a table may be made once for all, and from this θ may be found by inspection.

The wave-lengths made use of in this paper are generally those corresponding to lines A and H. These are both within the visible spectrum, yet far enough apart to render errors of observation of little consequence. In the case of the liquids examined I am altogether indebted to Dr. Gladstone, who furnished me with those contained in Table III. for the purposes of this investigation. When the line B is given, the octave ray to H may be obtained from A by interpolation, and these values were calculated by this method. It gives results sufficiently near the truth, but has been verified in case of CS_2 (temp. $1^{\circ}\cdot5$) and mint hydrocarbon by reference to the two indices A and H, so that the table depends on these two indices only. It will be seen that in the cases examined $\frac{\nu-1}{d}$ is constant, within the limits of observation, unaffected by temperature.

Dr. Gladstone and myself, in our joint paper, read before the Royal Society, May 6, 1863, and printed, p. 317, in the Transactions, have pointed out that in consequence of an accidental relation amongst the coefficients used in Cauchy's series, that the equation

$$\nu=\mu_H-3\ (\mu_F-\mu_B)$$

holds with considerable accuracy in the cases examined. It appears generally to give a fair approximation to the limit*, and thus to interpolate indices within the visible spectrum.

While, however, this investigation was in progress Prof. Langley's first paper on the determination of wave-lengths in the invisible spectrum appeared (Phil. Mag. March 1884). He there gives the wave-lengths of lower heat-rays down to length 2·030, and shows that the observed indices of refraction are not only below the limit obtained by Cauchy's formula, but even below those found by other formulæ. The material of his prism was flint glass. I found that the values derived from θ_H and θ_{2H}, while fairly accurate in the visible spectrum, were increasingly discordant in the longer wave-lengths. If θ_H were increased the lower indices were found more accordant; but then the visible spectrum was not adequately represented within the limits of any error which could be imagined possible. This seemed to throw a doubt on the existence of a limiting value. It appeared, however, that the glass prism absorbed the longer waves considerably. The

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* The divergence in case of CS_2 is, however, considerable, and ν is too large in comparison with that given by the above equations. Especially does this occur in highly dispersive media.

Phil. Mag. for May 1886 contained another paper by Prof. Langley, the material of the prism being rock-salt. On comparing the values found by observation with those furnished by the above equations, the agreement was sufficiently close to encourage investigation, and, accordingly, the calculations were carried further, the approximation being to single seconds in the value of θ and every value found.

The results are embodied in Tables I. and II. It will be seen in the case of rock-salt that the agreement between the values obtained from the sine—which involve only wave-lengths in free æther—and those obtained from the arc, which involve the two indices, and which should be equal to the former, are very close. The errors, it will be observed, are of small amount and not of uniform sign, possibly, however, showing a small tendency to increase on the side of angle from the sine in the case of the lower indices. Turning to the table of indices in the case of flint glass, we see that the angle for H, 24° 18′ 40″, gives fairly accordant values down to wave-length 0·940, and not far from the truth to 1·270; after this the values increase in the case of the sine over that of the arc, or, in other words, the calculated index comes out too large. This is the more important because the proportionate increase on so small an angle as 4° makes a large increase in the resulting index. If the value of θ_H be increased, this difference is diminished in the lower values; but it would require an increase far greater than could possibly be allowed as due to errors of observation in order to include the lower values, and the upper would become greatly discordant with observation.

It was, however, noticed by Prof. Langley that the flint-glass prism appeared strongly absorbent of the lower rays. It would no doubt be unsafe to reason very confidently from results obtained from only two substances. We may remark, however, that the approach to limit of refraction, if it exist, must display itself in observation by a small increase in the index for a large proportionate increase in the wave-length. Any disturbing cause then must become, proportionately, also increasingly effective as the wave-length increases. Hence must arise a tendency to mask the limiting value if attempted to be found by observation. Now in the case of flint glass we can hardly suppose but that some effect is produced by the change of temperature which results from the passage of the heat-rays through the medium. In the case of fluids the heat lowers the index, and thus the limit would be lowered also, and in somewhat greater proportion. It is, in the absence of sufficient data, not easy to say what would be the effect of this change of temperature in solids. In the case of glass, the *course* of the ray through the medium would possibly be raised in temperature above the surrounding mass; and thus there would be a cylinder of glass through which the pencil of rays passed, which might be in a state of constraint as compared with that around it. That a state of constraint exists is shown by the glass "flying" when suddenly heated.

However this may be, it seemed desirable in the first place to continue the investigation with the data at hand, and to calculate the value of the limit as given by the above equations, using as data two observed indices only. The results are given in the tables, and will be seen to include a considerable number of fluids differing widely in specific gravity, optical properties, and chemical constitution. The list it will be noticed includes certain isomeric bodies and also several differing by a single component.

The first fluid examined was bisulphide of carbon at different temperatures, the data for which were furnished by Dr. Gladstone to me for the purpose of this investigation. It will be seen that the quantity $\frac{\nu-1}{d}$ is practically a constant. The same result is observable in the case of mint hydrocarbon and benzene. The range of temperature is not indeed very large, but within the limits taken it is evident that the specific gravity and the quantity $\nu-1$ increase or diminish *pari passu*, and there is but little doubt that this relationship obtains generally. It would be very desirable indeed to have longer ranges of temperature * if these could be procured without corresponding errors which high temperature would almost certainly have a tendency to introduce.

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* This is especially desirable near boiling and freezing points. It is possible that the specific gravity at boiling-point might reveal relations at present masked by the arbitrary character of specific gravities taken at any temperature convenient for observation.

It is of interest evidently, in the case of a highly dispersive substance, to determine how far the different indices yield the same limit, as they clearly should do if the formulæ are exact. For the determination of this a value of θ was chosen which should yield the value which would satisfy θ_A; and then wave-lengths of lines B C D E F and G in free æther and in the medium were treated so as to find θ_B &c. If the sine and arc give the same result, then calculation and observation agree. It will be seen that in the case of CS_2 there is an outstanding difference which amounts to not quite three minutes of arc, and increases from A to H. This is equivalent to saying that the formulæ give correct results to the third place in the case of CS_2. This is not so good as the rock-salt in Table II. The index of G is most affected. The difference was noticed by the late Professor Baden Powell, in his 'Undulatory Theory as applied to the Dispersion of Light,' London, 1841. This matter deserves further investigation, but it is a matter also which will involve a considerable amount of calculation; thus it seemed right to postpone this for the present, as being better policy to obtain first a sufficient number of values of ν in case of media varying considerably in optical properties.

It seemed also desirable to include in the choice of bodies several which were isomeric. There is a certain analogy in these to a body at different temperatures, as in either case we are dealing with the same chemical elements. The result is, as will be seen from examining the tables, that in many cases $\frac{\nu-1}{d}$ is very nearly a constant. In no case yet examined is the divergence very great. Thus in benzene and styrolene, cresol, metacresol, and benzil alcohol, benzil chloride and chlorotoluine, methyl citraconate and methyl mesaconate, the agreement is close; but in case of picoline and aniline, as also acetone—with which butyric ether agrees—as compared with allyl alcohol there is a difference of about three units in the second place *. It may be merely an accidental coincidence, but it appears that the quantity $\frac{\sqrt{h}}{d}$ in these two last-mentioned cases is nearly the same, but in

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* But in these latter the specific gravity is evidently too high in the case of acetone, and in a less degree with butyric ether.

others the divergence is found to be greater. This also requires further investigation.

It must, however, be remembered that in all these the limit is derived solely from A and H. It is evident, then, that if either of these indices for A or H are in error, or *are affected by anomalous dispersion*, all the rest will be affected. Yet, considering the nature of the investigation,—which is, given certain indices to find the rest—it is evident that the fewer which are assumed as data the more confidence we may have, if we find that the rest are all calculable within reasonable limits. This, then, is the result of the present investigation: given two indices, all the rest can be found within limits, which are in a considerable number of instances very fair approximations to the truth. As, however, in the cases observed the outstanding differences are in the same direction, and increase apparently towards the more refrangible end of the spectrum, we have an indication of law, which may be sought for both in the circumstances of the experiment and in the mathematics of the problem.

It will be observed that the effect of substitution on the quantity $\nu-1$ is very marked indeed. This is shown in the case of ethyl sulphide and ethyl thiocarbamide, and still more in that of the substitution of iodine and bromine for chlorine. Iodine is peculiarly effective in increasing the specific gravity in far greater proportion than it raises the value of the refractive and dispersive powers. No simple relation between either density and $\nu-1$ or the quantity denoted by h is as yet apparent. I have tried the square and cube, and the square root and cube root of h, as well as the simple power. I have noticed above that $\frac{\sqrt{h}}{d}$ is in case of some isomeric bodies nearly a constant, but this may be a mere coincidence. It is, however, worth noticing, as $\sqrt{h}$ enters the coefficient k of Airy's equation cited above. When the logarithms are at hand, the calculations are so simple that even in the absence of any theoretical considerations it seemed worth while to try them.

It will be observed that the equation, by (ζ) and (η),

$$a \sin \theta = \sin m\theta$$

is impossible if $a \sin \theta$ is greater than unity. Now as a is the ratio of two wave-lengths in free æther, denoted in this investigation by $\lambda : \lambda_1$, we may ask what happens if the ratio of λ_1 the upper wave-length to λ the lower exceeds the ratio unity. Now in all cases yet examined, with the exception of selenium, this shorter wave-length is beyond violet and indeed the visible spectrum. In selenium it must lie somewhere near E; in solid phosphorus and sulphur it is beyond the furthest limit of the spectrum towards its more refrangible end, given by Prof. Langley, *i. e.* 0·3727. The results are given for these three substances in Table IV. annexed. It will be seen that phosphorus reaches the limit at about 2684 and sulphur at nearly 2664. We may observe as a coincidence that the behaviour of selenium in relation to light is remarkable, while its red colour shows that the violet end of the spectrum is absorbed. The results in Table V. are supplemental, calculated to the nearest minute, from determinations recently supplied by Dr. Gladstone, of ν for water and alcohol.

Abstract of Results.

1. That the relationship between the limit of refraction (ν) found by the equation $a \sin \theta = \sin m\theta$, where a is the ratio of two wave-lengths in free æther and m the corresponding ratio in the medium, give a value which has the property expressed by the equation $\frac{\nu - 1}{d}$ a constant, where d is the density of the medium and ν its limit of refraction at the same temperature.

2. That in isomeric bodies the same relation obtains closely in a large number of the bodies examined.

3. In all the cases examined ν is a quantity not *very* far below μ_A.

4. That the substitution of one chemical element for another has a very great influence on the quantity $\frac{-1\nu}{d}$, and substances with high chemical equivalent have, as far as examined, a far greater influence in increasing the density than in raising the limit.

5. That the equation $a \sin \theta = \sin m\theta$ gives in the cases

examined an approximate value for all the other indices, two being assumed as data. In the case of rock-salt this holds good far down in the invisible heat-spectrum, and also in the ultra-violet, but fails somewhat at a certain point in the flint-glass spectrum, and also, to a less degree, at the upper end of the visible spectrum of bisulphide of carbon.

It is evident that for a certain value of a and θ the equation $a \sin \theta = \sin m\theta$ becomes impossible. This would mean that there is an upper limit which is given by a certain value of wave-length, beyond which the formula gives impossible results. In the case of selenium only, of the substances examined, is this limit within the visible spectrum. The analogy with the critical angle of total reflexion gives this circumstance considerable interest.

It is evident, then, in the present state of our data, that in all cases examined the formula of Sir Geo. Airy gives an approximate value of an index by means of two others, and in this way they can be calculated; but that, as a rule, the lower indices come out too high as compared with the upper. A cause for this may perhaps be suggested in the heating of the medium due to the absorption of the rays; and as there seems to be evidence of absorption of short waves above the longer ones, given by recent stellar photometric determinations, this absorption would perhaps, if included in the conditions of the problem, account for some of the outstanding difference. This and anomalous dispersion —itself apparently depending on absorption—opens an interesting field of inquiry, and in this direction I propose to pursue the investigation.

Since the above was written I have been informed by Dr. Gladstone that he in some cases used a screen of solution of alum, but found that practically nothing was gained by its use. This fact, then, must be taken into account in any attempt to explain the outstanding differences between theory and observation.

TABLE I.—LANGLEY'S FLINT-GLASS PRISM.

Line.	Wave-length.	Index.	θ from sin.	θ from arc.	θ from sin.	θ from arc.
O......	0·34400	1·6266	28° 21′ 6″	28° 22′ 50″	29° 16′ 30″	29° 17′ 10″
H ...	0·39679	1·6070	**24 18 40**	do.	**25 5 0**	
F......	0·48606	1·5899	19 38 10	19 38 7	20 14 50	20 15 33
D......	0·58890	1·5798	16 6 20	16 6 26		
C......	0·65618	1·5757	14 24 57	14 24 57	14 51 15	14 52 25
A......	0·76009	1·5714	12 24 20	12 24 20		
	0·815	1·5697	11 33 46	11 33 42		
	0·850	1·5687	11 4 50	11 4 43		
	0·890	1·5678	10 34 30	10 34 28		
	0·910	1·5674	10 20 30	10 20 23		
	0·940	1·5668	10 0 21	10 0 21		
	1·130	1·5636	8 18 43	8 18 23		
	1·270	1·5616	7 23 19	7 22 53		
	1·360	1·5604	6 53 56	6 53 15		
	1·540	1·5576	6 9 30	6 8 31	6 20 30	6 20 13
	1·580	1·5572	5 56 0	5 54 20		
	1·810	1·5544	5 10 40	5 9 10	5 19 50	5 19 7
	1·870	1·5535	5 0 40	4 59 10		
	1·980	1·5520	4 43 55	4 42 20		
	2·030	1·5515	4 37 11	4 35 17	4 44 32	4 41 1

TABLE II.—ROCK-SALT PRISM.

(Prof. Langley's Determinations, Phil. Mag.
May 1886, no. 132.)

Line.	Wave-length.	Index.	θ from sine.	θ from arc.	Difference.	Limit of Refract.	
						Sine.	Arc.
M ...	0·3727	1·57486	24° 46′ 35″	24° 46′ 45″	+0′ 10″		
L......	0·3820	1·57207	24 8 5	24 8 19	0 14		
H_2 ...	0·3933	1·56920	23 23 50	23 24 8	0 18		
H_1 ...	0·3968	1·56833	**23 10 49**	datum.		1·52570	
G ...	0·4303	1·56133	21 17 0	21 17 7	0 7		
F......	0·4861	1·55323	18 44 47	18 44 47	0 0		
b_4 ...	0·5167	1·54991	17 35 41	17 35 40	−0 1		
b_1 ...	0·5183	1·54975	17 32 19	17 32 16	−0 3		
D_1 ...	0·5889	1·54418	15 22 49	15 22 49	0		
D_2 ...	0·5895	1·54414	15 21 50	15 21 52	+0 2		
C......	0·6562	1·54051	13 46 12	13 46 12	0		
B......	0·6867	1·53919	12 8 49	12 8 50	+0 1		
A......	0·7601	1·53670	11 51 12	11 51 13	+0 1		
ρ......	0·94	1·5328	9 33 53	9 33 58	+0 5		
φ......	1·13	1·5305	7 56 42	7 56 40	−0 2		
ψ......	1·38	1·5287	6 29 49	6 29 51	+0 2		
Ω......	1·82	1·5268	4 55 24	4 55 14	−0 10	1·5251	1·5249

H_1 is found from indices of H_1 and A, and from H all the other lines are deduced. Molecular distance $h = 0{\cdot}032570$; $\mu_H - 3(\mu_F - \mu_H) = 1{\cdot}52621$.

TABLE III.

Bisulphide of Carbon.

Temp. t.	Sp. Grav. d.	Index A. μ_A.	Index H. μ_H.	Angle A. θ_H.	Limit. ν.	Mol. dist. h.	Specific Limit. $\frac{\nu-1}{d}$.
1°·5	1·2909	1·6227	1·7159	18° 38′ 2″	1·5944	·048505	46048
23·0	1·2594	1·6070	1·6972	18 25 50	1·5799	·04844	46046
38·0	1·2494	1·6026	1·6922	18 25 30	1·5752	·048559	46038

Another Specimen.

Temp. t.	Sp. Grav. d.	Index A. μ_A.	Index H. μ_H.	Angle A. θ_H.	Limit. ν.	Mol. dist. h.	Specific Limit. $\frac{\nu-1}{d}$.
10·0	1·2793	1·6153	1·7078	18 37 43	1·5869	·048673	45885
24·5	1·2593	1·6045	1·6954	18 30 45	1·5770	·048616	45819

Comparison of Values of Limit calculated from sine θ_H and arc θ_H with corresponding angles for CS_2 at 10°.—$\theta_H = 37° 44' 40''$.

	Line A.	B.	C.	D.	E.	F.	G.
Sine ...	18° 37′ 43″	20° 42′ 50″	......	24° 19′ 59″	......	29° 54′ 4″	34° 19′ 40″
Limit...	1·5870	......	......	1·5862	......	1·5843	1·5847
Arc ...	18 39 46	20 41 52	......	24 18 52	......	29 55 58	34 16 52
Limit...	1·5869	......	......	1·5861	......	1·5849	1·5848

Another value obtained by interpolation, $\theta_H = 37° 29' 20''$.

Sine ...	18° 35′ 20″	20° 40′ 10″	21° 40′ 40″	24° 16′ 50″	27° 23′ 30″	29° 54′ 50″	34° 13′ 40″
Arc ...	18 35 10	20 40 40	21 40 0	24 15 30	27 21 10	29 51 50	34 11 10

The value of ν lies between 1·5867 and 1·5852.

Mint Hydrocarbon.

Temp. t.	Sp. Grav. d.	Index A. μ_A.	Index H. μ_H.	Angle A. θ_A.	Limit. ν.	Mol. dist. h.	Specific Limit. $\frac{\nu-1}{d}$.
6°·5	0·8926	1·4728	1·4988	11° 4′ 50″	1·4632	......	51893
25·0	0·8820	1·4650	1·4901	10 58 0	1·4561	......	51712

Benzene.

Temp. t.	Sp. Grav. d.	Index A. μ_A.	Index H. μ_H.	Angle A. θ_A.	Limit. ν.	Mol. dist. h.	Specific Limit. $\frac{\nu-1}{d}$.
2·0	0·8979	1·5021	1·5460	13 56 40	1·4873	·039219	54272
23·7	0·8760	1·4893	1·5320	......	1·4725	·039122	54328
28·6	0·8709	1·4860	1·5279	13 50 35	1·4720	·038892	54196

Table III. (*continued*).

Benzene.—Another Specimen.

Temp. t.	Sp. Grav. d.	Index A. μ_A.	Index H. μ_H.	Angle A. θ_A.	Limit. ν.	Mol. dist. h.	Specific Limit. $\frac{\nu-1}{d}$
10°·0	0·8868	1·4935	1·5355	13° 43′ 10″	1·4792	·038802	54037
21·5	0·8773	1·4887	1·5304	13 41 45	1·4745	·038850	54077
Styrolene.							
11·0	0·9409	1·5208	1·5693	14 1 0	1·5057	·038848	53747
Another Specimen.							
							53853
Cresol.							
23·0	1·039	1·5316	1·5787	14 45 0	1·5137	·040776	49442
Metacresol.							
19·0	1·033	1·5257	1·5726	14 13 50	1·5102	·039395	49390
Benzil Alcohol.							
22·0	1·0412	1·5278	1·5710	13 58 0	1·5150	·038089	49467
Methyl Citraconate.							
15·5	1·1164	1·4442	1·4721	11 32 0	1·4345	......	38920
Methyl Mesaconate.							
16·0	1·1246	1·4492	1·4813	12 19 0	1·4380	.....	38938
Picoline.							
23·5	0·94093	1·4912	1·5317	13 29 0	1·4774	·038247	50726
Aniline.							
13·0	1·016	1·5695	1·6336	16 9 0	1·5488	·043342	54016
Another specimen.							
7·5	1·0322	1·5780	1·6449	16 25 0	1·5565	·043940	53920
Benzil Chloride.							
7·0	1·099	1·5314	1·5764	13 59 0	1·5160	0·37148	46592
Chlorotoluine.							
19·0	1·0761	1·5173	1·5613	13 54 0	1·5027	·038701	46717

Table III. (*continued*).

Temp. t.	Sp. Grav. d.	Index A. μ_A.	Index H. μ_H.	Angle A. θ_A.	Limit. ν.	Mol. dist. h.	Specific Limit. $\frac{\nu-1}{d}$.
Acetone.							
*25·5°	0·8117 at 15°	1·3540	1·3706	9° 25′ 0″	1·3479	·024733	42860
Butyric Ether.							
*23·0	0·8778 at 20°	1·3850	1·4669	9 24 0	1·3778	·027995	43039
Allyl Alcohol.							
23·0	0·8563	1·4054	1·4289	10 44 55	1·3695	·025438	46304
Allyl Sulphide.							
11·0	0·8544	1·4531	1·4811	11 30 40	1·4434	……	51837
Ethyl Thiocarbamide.							
18·0	1·0030	1·5040	1·5477	13 13 0	1·4890	……	48794
Orthobromotoluine.							
……	1·4192	1·5502	1·5981	14 3 50	1·5299	……	37340
Compare this with benzil chloride above					1·5160	……	46592
Isobutyl Chloride.							
19·0	0·8626	1·3939	1·4111	8 57 30	1·3882	……	45004
Isobutyl Iodide.							
……	1·5982	1·4874	1·5240	12 53 0	1·4749	……	29717
Mercuric Methyl.							
30·0	2·966	1·5229	1·5683	14 5 0	1·5076	……	17365
Bromopicrin.							
13·0	2·816	1·5736	μ_D 1·5831	14 0 0	1·5246	……	18612

* It will be seen that in both these d is too great.

TABLE IV.

Substance.	Lower Index.	Upper Index.	Angle θ_A.	Limit.	Limiting Wave-length.
Selenium	μ_A. 2·653	μ_D. 2·980	θ_A. 44° 9′ 0″	ν. 2·1353	5295·7
Solid Sulphur	μ_A. 1·9024	μ_E. 1·9527	20° 30′ 0″	1·8621	2663·8
Solid Phosphorus	μ_A. 2·1059	μ_E. 2·1442	20° 40′ 0″	2·0611	2683·8

TABLE V.

Sp. g.	μ_A.	μ_H.	ν.	$\frac{\nu-1}{d}$.
Water. 1·000	1·32924	1·34393	1·3249	3249
Alcohol. 0·7919	1·3585	1·3735	1·3531	44589

XXV. *The Temperature at which Nickel begins to lose suddenly its Magnetic Properties.* *By* HERBERT TOMLINSON, *B.A.*[*]

IT has long been known that nickel, like iron, begins at a certain temperature to rapidly lose its magnetic properties, and that the critical temperature for the former metal is much lower than for the latter. According to Faraday, nickel loses its magnetic permeability about 330° to 340° C.; according to Becquerel, about 400° C.; according to Pouillet, about 350° C.; and according to Chrystal, about 400° C. Berson, however, seems to have been the first[†] to publish a curve showing the relation between magnetic induction and temperature right up to the point at which the former ceases practically to exist. The author has also, independently of Berson, drawn up curves of a similar kind which have not as yet been published, and which he ventures to offer to the Physical Society, because they not only supplement Berson's results, but they seem also to partly explain why different observers have obtained such widely differing temperatures for the point of *nil* permeability[‡].

In the axis of a magnetizing solenoid, and perpendicular to the magnetic meridian, was placed a nickel wire, 30 centim. in length and 0·0053 square centim. in section. The solenoid consisted of cotton-covered copper wire of $\frac{1}{20}$ inch in diameter, wrapped in a single layer of 8·25 turns to the centimetre round a brass tube slit throughout its entire length, and having an internal diameter of 2·5 centim. Before being wrapped round the brass tube the wire was well coated with pipeclay moistened in water. Round 10 centim. of the central portion of the solenoid were wrapped 240 turns of another piece of the same wire, also coated with pipeclay, to serve as a secondary coil. The whole was then introduced into an air-chamber and the wet pipeclay allowed to dry, at first slowly at the temperature of the air, and afterwards more quickly at temperatures which were gradually raised to 400° C. The air-chamber consisted of two concentric copper cylinders 40 centim. in length, enclosing between them an annular space about ·5 centim. thick which was filled with fine sand, and was heated by a row of burners placed underneath. The temperature of the air-chamber was calculated from the alteration of electrical resistance of a coil of platinum wire, whose resistance had been very carefully determined at different temperatures up to 100° C., and expressed in terms of the temperature by a formula of the form

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[*] Read February 25, 1888.

[†] *Ann. de Phys. et de Chim.* vol. vii. (1886).

[‡] That is *practically* nil. The experiments of Faraday seem to show that the permeability never *entirely* vanishes.

$$R_t = R_0(1 + at - bt^2),$$

where R_t and R_0 are the resistances at t° C. and 0° C. respectively, and a and b are constants. The platinum coil was wound double and was placed inside the slit brass tube close to the nickel wire, but insulated from it and the tube by asbestos; the length of the coil was 10 centim., and it occupied the central portion of the air-chamber. To the ends of the coil were hard-soldered stout copper terminal rods, which passed through a wooden cap closing one end of the air-chamber, and served to connect with a Wheatstone-bridge arrangement employed for determining the resistance of the platinum. The other end of the air-chamber was also closed with a wooden cap, through holes in which passed the ends of the wires of the magnetizing solenoid and the secondary coil. After the pipeclay coating had become thoroughly dry the resistances of the primary and secondary coils were tested, and found to be sensibly the same as before winding. The object of coating the cotton covering of the wires with pipeclay was to maintain the insulation, for at the temperatures reached in some of the experiments the cotton became charred. Whilst the coils lay undisturbed the pipeclay coating served the purpose for which it was intended very well; but when they were removed from the air-chamber on the conclusion of the experiments, both the charred cotton and the pipeclay easily came away from the wire, and so rendered the coils useless.

A pair of primary and secondary coils exactly similar to the first, except that they had no pipeclay coating and no nickel core inside, was kept buried in a box filled with well-dried sawdust and placed about two feet away from the air-chamber. The two primary coils were connected up in series with each other, a battery, a key, a rheostat, and a tangent-galvanometer whose constant had been previously carefully determined. The secondary coils were also connected up in series with each other, with a rheostat, a Thomson's reflecting-galvanometer, and an earth-coil. The earth-coil consisted of seven turns of silk-covered copper wire laid side by side round the circumference of a wooden disk 30 centim. in diameter; the disk was supported with its plane horizontal, but could be turned round a horizontal axis in either direction through an angle of 180 degrees. The ends of the wires round the disk were soldered to amalgamated copper disks revolving in mercury-cups, which last served to connect the earth-coil with the rest of the apparatus in series with it. The two pairs of primary and secondary coils were so arranged that the currents induced on opening or closing the battery-circuit by means of the key exactly balanced each other before the introduction of the nickel into one of them. When therefore the nickel was introduced, the observed deflection produced by opening or closing the primary circuit was entirely due to the magnetic permeability of the nickel; and by comparing this deflection with that produced by turning the earth-coil suddenly through 180° the permeability could be determined in absolute measure, since the vertical component of the earth's magnetic force at the place was known. The earth-coil also served another purpose, namely to secure with the aid of the rheostat the constancy of the sensitiveness of the Thomson-galvanometer. As the temperature of the air-chamber was raised, the total resistance in both primary and secondary circuits increased; but by altering the rheostats in these circuits until the deflection of the tangent-galvanometer in the one case, and of the Thomson-galvanometer, when the earth-coil was suddenly turned, in the other, became the same as before the heating, constancy both in the magnetizing force and in the sensitiveness of the Thomson-galvanometer was secured.

The mode of proceeding was as follows:—By means of the set of burners underneath the air-chamber the temperature was raised to nearly the highest point which it was desirable to attain; the burners were then adjusted until the temperature was either constant or very slowly rising or falling; this could always be secured by waiting for a sufficient length of time. The rheostat in the secondary circuit was then adjusted until the deflection of the Thomson-galvanometer, produced by suddenly turning the earth-coil, was exactly 80 divisions of the scale. The rheostat in the primary circuit was next adjusted until the required current had been reached, care being taken to begin with a small current, which was increased very gradually to the required amount. As it was the aim of the author to test only the *temporary* permeability* of the nickel, the current in the primary circuit was opened and closed a great many times, until the current induced on closing the circuit became the same in magnitude as that induced on opening the circuit; and then the mean of the deflections produced by ten times closing the circuit was taken to measure the temporary induction. The air-chamber was now allowed to cool a little, and then, as before, after the temperature had been steadied by adjusting the burners, a fresh set of observations was made, and so on until the temperature of the room was reached. As soon as the observations with the lowest magnetizing force had been completed, fresh ones were made with a higher magnetizing force until a magnetizing force of 18·183 C.G.S. units had been reached.

The temporary permeability of the nickel was calculated from the following formulæ:—

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* It is impossible to obtain *true* relations between change of temperature and change of *total* permeability by the "ballistic method," because the mere act of changing the temperature shakes out some of the sub-permanent magnetism which the metal may have acquired at previous temperatures.

$$M_f = \frac{4\pi n_1 C}{l}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$M_p = \frac{2NAVd}{DSn_2M_f}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

In formula (1) M_f is the magnetizing force, n_1 is the number of turns in the primary coil, l the length of the coil, and C the current circulating round the coil.

In formula (2) M_p is the magnetic permeability (ratio of magnetic induction to magnetizing force), N the number of turns in the earth-coil, A the area of the earth-coil, D the deflection produced by suddenly turning the earth-coil through 180°, V is the vertical component of the earth's magnetic force, d the induction-current due to the nickel, S the section of the nickel, and n_2 the number of turns in the secondary coil. C.G.S. units were used throughout. Since the length of the nickel under the magnetizing force was about 350 times the diameter, the effect of the ends is quite negligible. The results are given in the following table, which is supplemented by the curves in fig. 1. In fig. 2 are given Berson's curves for nickel, and in fig. 3 similar curves obtained by Ledeboer* for iron.

TABLE I.

Magnetizing force.	4·959	6·612		9·918		11·571		18·183	
Temperature, in degrees Centigrade, t.	Permeability, M_p.	t.	M_p.	t.	M_p.	t.	M_p.	t.	M_p.
16·5	93·4	19·5	88·8	20	88·6	19	87·6	18	82·0
84·5	127·9	55·0	97·1	54	93·4	47	91·1	60	84·4
117·6	125·7	93	101·0	74	93·4	84	89·8	79	87·6
128·0	125·0	127	97·6	83	94·1	133	95·2	102	90·8
160·0	126·4	160	107·0	102	95·5	178	97·4	143	89·6
200·0	127·9	186	108·1	143	102·9	191	101·5	174	91·6
227·0	130·9	227	120·7	157	103·7	275	105·6	186	92·6
261 0	141·2	279	126·3	204	106·3	286	104·0	254	92·4
282·0	144·1	301	111·4	247	111·8	295	100·9	293	90·2
297·0	142·6	349	60·7	262	112·1	330	63·0	316	72·2
304·0	139·7	……	……	275	112·9	346	54·2		
306·5	125·0	……	……	308	96·3				
317·0	102·9	……	……	340	65·1				

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* Both these and Berson's curves are taken from the 'Electrician,' vol. xx. No. 505.

Fig. 1.

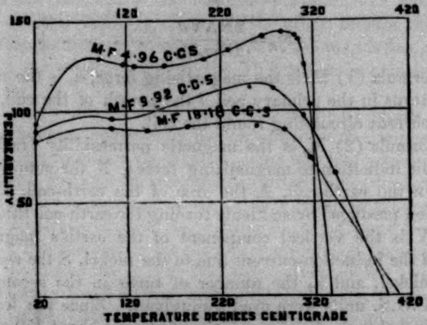
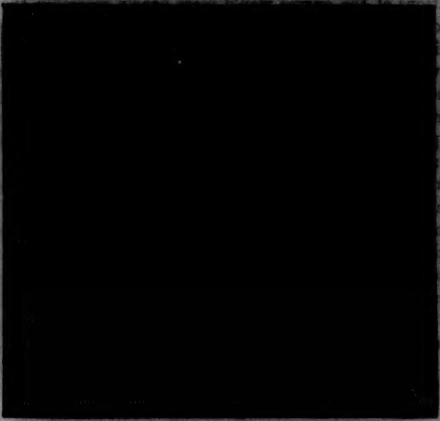


Fig. 2.

It seems from the above table and the curves in fig. 1 that the temperature of maximum permeability is lower the greater the magnetizing force. Thus, for the magnetizing forces 4·959, 9·918, and 18·183, we get the temperatures of maximum permeability as 287°, 248°, and 242° C. respectively.

The temperature at which the permeability practically vanishes seems, on the other hand, to be higher the greater

Fig. 3.



the magnetizing force. As for the above-mentioned magnetizing forces, we have for this temperature the values 333°, 392°, and 412° C. respectively. In this respect the behaviour of nickel resembles that of iron, as is evidenced by the curves in fig. 3 ; and it is probably partly for this reason that different observers have obtained different results for the temperature of *nil*-permeability. Another cause for the discrepancies in this respect between the observations of different experimenters is probably to be found in the want of purity of the metal; for it is evident that if iron be present as an impurity, the point of *nil*-permeability may be rendered very much higher*. The observations made by the author were not in any case carried right up to the vanishing-point of the permeability, as the nickel used by him was, though nearly, not quite pure†;

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* The temperature at which the permeability of iron vanishes seems, from Ledeboer's curves, to lie between 750° and 770° C.

† The wire was procured from Messrs. Johnson and Matthey, who informed the author that they found it impossible to draw pure nickel wire. Mr. G. S. Johnson, the Demonstrator of Chemistry at King's College, London, has kindly furnished an analysis of the wire. It contains 97·5 per cent. of nickel and only 0·67 per cent. of iron.

and it is not at all unlikely that, had this been done, the terminations of the curves would more nearly resemble those of Berson, shown in fig. 2, where it will be noticed that there is a point of inflexion before 320° C. The author does not believe that, even with perfectly pure nickel, the magnetic permeability would *entirely* vanish; but the following experiment will show that, with a thin layer of pure nickel, the permeability *practically* vanishes with great suddenness:—A brass wire 1 millim. in diameter was coated with a thin layer of nickel by electrolysis, and a piece of it, about 2 centim. in length, was suspended horizontally in a cradle of platinum between the pole-pieces of a powerful electromagnet in a direction perpendicular to the lines of magnetic force; the platinum cradle was supported by a platinum wire 12 inches long and $\frac{1}{100}$ inch in diameter. The nickel-plated brass wire was heated by a burner to a temperature of visible red, and the electromagnet was then excited by five Grove-cells. Not the slightest effect of the intense field seemed to be experienced by the nickel, even after it had been cooling for several seconds. The magnetizing circuit was now broken, leaving only the residual magnetism in the pole-pieces, when, after a few seconds of further cooling, and without *the least preliminary warning*, the wire set axially with startling rapidity.

The chief aim of the author in this research was to fix approximately the temperature at which the rate of loss of permeability begins to be greatest; and this appears to be 300° C., not only for the specimen of nickel used by him, but also for that used by Berson, and this, too, both for the temporary and total magnetization. The following table shows the rate at which the temporary permeability decreases with rise of temperature between 300° and 320° C.

From this table it is apparent that the rate of decrease of permeability is less the higher the magnetizing force; so much so indeed that whereas, with a magnetizing force of about 5 C.G.S. units, there is for each degree rise of temperature a decrease of 11 lines of force per square centimetre, with a force of 18 the loss of lines per degree is only increased to 14.

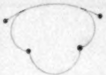


Fig. 1.

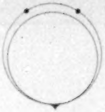


Fig. 2.

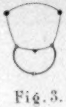


Fig. 3.

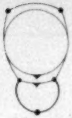


Fig. 4.

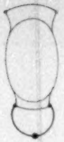


Fig. 5.

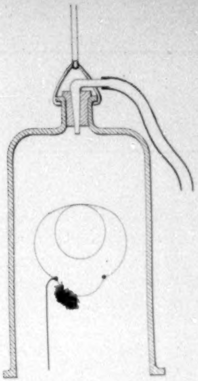


Fig. 9.

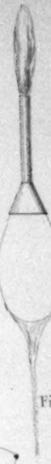


Fig. 10.

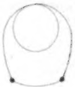


Fig. 6.

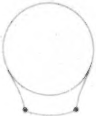


Fig. 7.

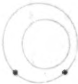


Fig. 8.

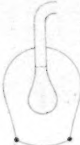


Fig. 11.

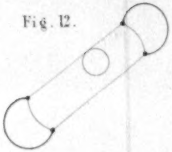


Fig. 12.

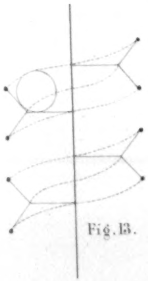
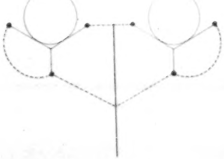


Fig. 13.

Fig. 14.



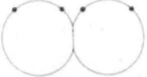


Fig. 16.

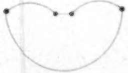


Fig. 17.

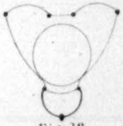


Fig. 18.

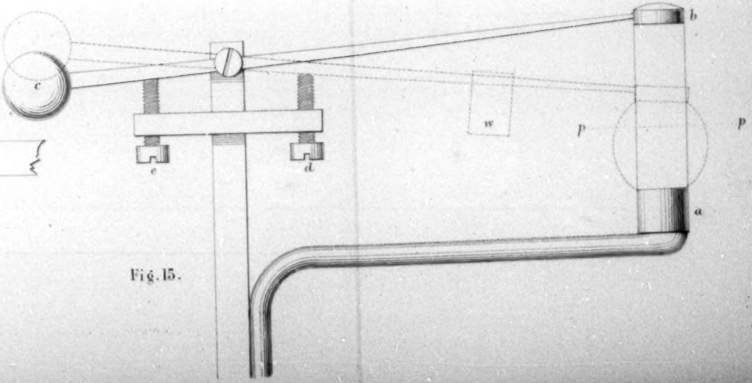


Fig. 15.

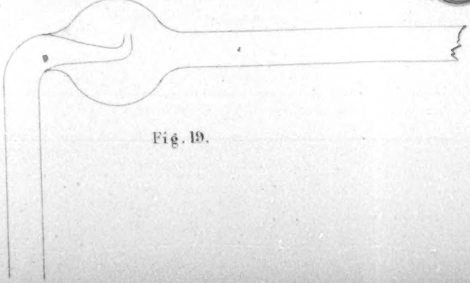


Fig. 19.

Mintern Bros. lith.

Table II.

Temperature, in degrees C., t.	Magnetizing force, M_f.	Permeability, M_p.	Differences between values of M_p at 300° and 320° C. divided by 20, $\frac{\Delta M_p}{\Delta t}$.	$\frac{\Delta M_p}{\Delta t} \times M_f$.
300	4·959	141·0	2·22	11·1
320		96·6		
300	11·571	95·5	1·08	12·5
320		73·8		
300	18·183	84·8	0·79	14·3
320		69·1		

XXVI. *Experiments with Soap-bubbles.* By C. V. Boys, *A.R.S.M., Demonstrator of Physics at the Science Schools, South Kensington**.

[Plate VII.]

Though none of the experiments I am about to describe depend upon any property of a soap-film which is not perfectly well known and understood, yet they serve to illustrate in a striking and beautiful manner the behaviour of bubbles under special circumstances, and so as lecture-experiments simply I hope they may be considered worthy of the attention of the Physical Society.

Everyone is familiar with the fact that a soap-bubble may be supported or even struck by a piece of baize or wool without coming into real contact with the material; it is also well known that two bubbles supported on the pipes from which they are blown, or on rings, may be pressed or knocked together with such violence as to materially alter their shape, and yet they do not come into real contact; there is a film of air between them which they are unable to squeeze out. This film, though thin to ordinary tests, is so thick that the colours of Newton's rings are only seen when one of the bubbles is very small, so that the air is squeezed out the more readily. If the pressure is increased so as to make a real contact, the bubbles both instantly burst. That this pressure may be made great before the true contact takes place will be shown in a variety of ways hereafter; but the following simple experiment makes it very evident that the air-film will prevent the contact of two soap-films that are pressed together.

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* Read April 14, 1888.

Exp. 1.—Blow a bubble about 9 cm. in diameter, and place it on a ring with a diameter of about 7 cm. This bubble may be pulled or pushed through the ring by means of a smaller wire ring which serves as a handle. (See 'Nature,' 1871, p. 395.) It may be so adjusted that the weight of the ring will not pull it through. Then a ring larger than the bubble, carrying a plane film, can be used to push it up and down through the ring, and yet the two films do not touch (fig. 1, Pl. VII.).

Bearing this fact in mind, that two bubbles may press one another without true contact, I hoped to be able to blow and detach one bubble within another, and let it roll about within the larger bubble. This, however, is made difficult by the accumulation of a small quantity of solution at the bottom of each, the weight of which is able immediately to press through the air-film between them and so cause both bubbles to burst. However, the experiment can be performed in the following manner:—

Exp. 2.—Blow a bubble on the lower side of the same ring that was used in Exp. 1, and if a large drop does not remain hanging to the bubble slowly apply solution to any part until as great a drop as can safely be carried has accumulated. Then pass the end of the pipe through the upper side of the bubble, and blow another inside, but take care in this case to have no excess of liquid. When the inner bubble is about twice as large as the outer one was at first, remove the pipe with a rapid movement. The inner one will now fall gently and rest within the outer one, the heavy drop pulling the thick part of the outer bubble out of reach of the inner one. The air of the outer bubble may then be withdrawn until the space between the highest point of the two bubbles is no more than two or three millimetres (fig 2, Pl. VII.).

The great pressure which the air-film will carry is well shown by the next experiment, which, moreover, is more easily carried out than the last.

Exp. 3.—Proceed as in the last experiment, but instead of making a large drop on the first bubble, hang on a moistened ring of wire rather smaller than the fixed ring. This ring should be weighted until it pulls the bubble so much out of shape that a tangent to the curve at the points where the film meets the hanging ring makes an angle of 20° or 30° with the plane of the ring (fig. 3, Pl. VII.). A bubble may then be blown inside and allowed to drop, when it will be found to rest on the conical seat provided by the outer bubble, while the heavy drops of liquid are kept apart, and thus there is no fear of contact (fig. 4). These drops may now be both removed with the end of the blowpipe; then, if the lower ring is pulled down slowly, it will be found that the inner bubble is being squeezed out of shape until it becomes a beautiful oval, while the outer bubble shows the effect of the pressure by a corresponding enlargement (fig. 5). If the lower ring is pulled down still further, the outer bubble is simply pulled in half, and the inner one, often unbroken, gently floats away. This shows that contact was not made, as in that case both would be immediately broken. If, however, instead of pulling the ring too far, it is held in the position shown in fig. 5, it will be found that it is possible to swing the pair of bubbles round and round, and yet in spite of this violent treatment the bubbles refuse to touch one another. Or, if the lower ring is cautiously inclined and pulled away, the outer bubble will peal off it and remain attached to the upper one only. The two bubbles will now be spherical again, but there will be no heavy drop as in fig. 2. The air of the outer bubble may be withdrawn as before, until the two bubbles are barely separate.

This experiment, and many of those that follow, may be made more beautiful by using for the inner bubble a solution strongly coloured by fluorescine, or still better by uranine (for the knowledge of which I am indebted to Mr. Madan); then, if sunlight, electric or magnesium light is thrown on to the bubbles, the inner one appears a brilliant green, while the outer one remains clear as before.

The power of the surface-tension to do work is demonstrated by blowing a large bubble below the ring and hanging on the weighted ring. If now a very small ring, a centimetre or more in diameter, is placed on that part of the bubble which is stretched across either ring, and then the part within the small ring is made to burst, the air will escape through the small hole and the heavy ring will be lifted until it comes in contact with the upper one. If the film over the whole of the heavy ring is burst instead, the ring is pulled up so suddenly that it is difficult to follow it with the eye, and it strikes the upper ring with such violence that the noise is loud enough to be heard across a large room.

A suspended ring affords a simple and accurate means of measuring the surface-tension of the soap-film. A plane film is formed across a fixed horizontal ring and a light smaller ring is attached to the plane film, which is then broken within the smaller ring so as to leave an annular film only. Weights are then hung on to the suspended ring until the angle between the film and the plane of this ring approaches 90°. At this point equilibrium becomes unstable, and the lower ring falls away, but now both rings will be found to carry plane films, though the moment before neither did. On repeating the experiment a few times it will often be found possible to use such a weight that the ring will hang for some time, but will gradually sink, while the angle referred to above will approach more and more nearly to 90° as the surface-tension of the film diminishes; and thus the exact surface-tension at the particular moment of separation may be found by dividing weight of the ring and attached moisture by twice its circumference.

Exp. 4.—Bubbles blown with coal-gas are lighter than air and rise. If therefore an inner bubble is blown with such a mixture of air and gas as to rise, it will rest against the upper side of the outer bubble, where there are no heavy drops but where the films are thinnest and cleanest (fig. 6). A pair of bubbles blown in this way will sometimes last an hour when exposed to the air of the room. The inner bubble may be gradually enlarged by blowing in gas until the outer one can barely withstand the pull. The forms assumed under these circumstances are extremely graceful, and their beauty is increased by the play of colours on the two bubbles which the multiple reflexions seem to intensify. If, when the inner bubble is not too large, as in fig. 6, a little gas is gently let into the outer bubble, it is possible to so adjust the mixture of gas and air that the inner will float either near the top or near the bottom of the outer bubble, or about the middle, as may be desired (fig. 8). If under these conditions the bubbles are left undisturbed, the richer gas above the inner bubble will diffuse into the poorer and heavier gas below, and the bubble will slowly rise or fall, according to the relative quantities of gas and air. The diffusion through the film is well shown in the next experiment.

Exp. 5.—Blow a pair of bubbles, as shown in fig. 6, but make the inner bubble only just light enough to rest against the top of the outer one. Lower a bell-jar over all, and pass a stream of gas into the bell-jar by means of a tube passing through the top. As the air is gradually driven down, the outer bubble begins to feel the want of buoyancy, and gradually settles down, as shown in fig. 9. After a short time, the effect of the diffusion through both bubbles tending to enrich the gas of the outer bubble is made evident by the gentle descent of the inner bubble. If the jar is raised quickly, and a little air is blown into the outer bubble, it is possible to again cause the inner one to rise and float against the top of the outer one as before. The bell-jar may be lowered and the process repeated until the outer bubble is so large that the ring is unable to support its weight when in an atmosphere of coal-gas.

The very rapid diffusion of a vapour which will mix with the solution of which the film is made is easily shown.

Exp. 6.—Into a large inverted bell-jar pour a small quantity of ether, or to fill the jar with the vapour quickly wet a piece of blotting-paper with ether and stand it on edge in the jar. Remove the paper, then blow a bubble and drop it into the jar. The bubble will rest on the ether vapour as on carbonic anhydride, and while floating the most violent agitation of the colours of the film will be seen. The bubble does not remain floating long at the same level; it gradually sinks into denser and denser layers of vapour until it reaches the bottom or breaks on the way. This gradual sinking is due to the penetration into the bubble of the ether vapour, as may be shown as follows:—The bubble may be taken out of the vapour by means of a ring wetted with soap-solution and carried to a flame, when instantly there is a blaze of ether vapour a foot or more in diameter. That the flame is not due to liquid ether condensed on the film is shown by exposing a plane film to the vapour and carrying it to a light in the same way, when no trace of flame will be seen.

Exp. 7.—At the end of a wide tube, which has been enlarged at the lower end, blow a large bubble and lower it gently into the vapour of ether, holding the finger at the mouth of the tube. After a few seconds it will be found difficult to remove the bubble by means of the tube, because its weight may have become sufficient to tear it away when buoyed up by the air only. If it is removed successfully it will hang like a heavy drop; then, on removing the finger, a light may be put to the issuing vapour, which will burn like a bunsen-burner. If, moreover, the bubble full of ether vapour is held in a brilliant light, the shadow will show the ether vapour oozing through the film and falling away in a heavy stream (fig. 10). This experiment shows in succession the floating of an air-bubble on a heavy vapour, rapid diffusion of a soluble vapour through a soap-film, and the power of the surface-tension to force the heavy vapour up a tube fast enough to supply a large flame.

Exp. 8.—Blow a bubble with oxygen gas in a jar partly filled with ether vapour; on taking the bubble out of the vapour and carrying it to a light, it will explode with a loud report. Sufficient vapour will penetrate the bubble, even whilst it is being blown, to make the mixture violently explosive.

Exp. 9.—The weight of the air is well shown by blowing a bubble with gas on a ring and then trying to blow an air-bubble within it (fig. 11). The inner bubble is then pulled out into a pear-shape, and very soon breaks away from the pipe on account of its great weight.

Exp. 10.—If Exp. 4 be repeated, but instead of a heavy fixed ring a light aluminium one be used instead, to which is tied a long piece of thread which may have a sheet of paper at the end, then the whole combination will float and rise in the air, even though, as in fig. 7, practically the whole of the buoyancy is due to the gas in the inner bubble. In this case the inner bubble is the bag of a balloon, the outer bubble is the netting, and the wire and the things carried by it are the car. In this case the power of the air-film to resist contact of the two films is more evident than ever. If any of the former figures 6, 7, or 8 are carrying a wire ring and thread, as described, it is possible by a suitable pull at the thread to release the pair of bubbles, which float away, one inside the other, until the ceiling brings the experiment to a conclusion.

Exp. 11.—If the inner bubble of fig. 6 is made smaller than the ring, then the corresponding experiment to that represented in fig. 5 is shown in fig. 12. The small sphere will always roll to the upper end of the outer bubble, which may be pulled out to the cylindrical form and be inclined either way. This modification of the other experiment was suggested by Mr. Newth, to whom I had shown the previous combinations.

A great many experiments may be shown in which strings of two or more bubbles, filled some with air and some with gas, tend to pull in different directions. Thus an air-bubble with a gas-bubble blown on the top of it will rise till the gas-bubble breaks against the ceiling, when the air-bubble falls again, and may be sent up as often as desired by the addition of a new gas-bubble; or three bubbles, the lower one of air the upper one of gas, so proportioned that the combination just floats, will remain until the middle bubble is touched with the finger, when the other two immediately go opposite ways. There is no occasion to say more about experiments of this type.

Exp. 12.—An experiment which is easily performed shows in a striking way how the air-film resists being broken. If a pair of bubbles are blown as shown in fig. 4, and the vibrating prongs of a large tuning-fork are brought quite close to the line where one bubble rests upon the other, both films will take up the movement of the fork, and a point of light reflected by the two films is seen spread out into a pair of rings, so violent is the motion, yet the films do not touch. It is hardly possible to suppose that the two films remain as close together where the movement occurs as in other parts of the line of support; if they tend to separate they form an exception to the general rule that a vibrating body attracts an object in the immediate neighbourhood. In this case the inner bubble is heavier than the air in the outer one, both because of the weight of the film and the compression of the air within due to its tension. But if the same experiment is tried when the inner bubble is lighter than the air in the outer one, as it may be by holding one of the prongs close to the highest point of the bubbles shown in fig. 6, or when either bubble is heavier or lighter than air, the same result will be found—the bubbles will refuse to touch one another.

Plateau has described (*Statique des liquides*) a number of very beautiful experiments in which wire frames representing the edges of geometrical solids are dipped in soap-solution, after which they are found to carry combinations of films, plane or curved according to the character of the frame. Thus within a triangular prism, when it is removed from the solution, is found a combination of nine plane films which form three troughs meeting along the axis of the prism and a triangular pit at each end.

Exp. 13.—A spherical bubble may be dropped into one of these troughs and rolled from end to end, it may be taken out of one trough and dropped into another, or the frame may be held with its axis vertical, when the bubble may be dropped into the triangular pit, where, however, it will not remain long.

The characteristic feature of all the laminar figures is that there are never more than three surfaces meeting in a line where the angles are always 120°, or more than four lines or six surfaces meeting in a point. Further, the mean curvatures of the films are always zero so long as no air is enclosed. As Plateau mentions in his book, the screw-surface has no mean curvature, therefore if a frame be made out of a helix of wire with its ends connected by wire to a solid axis, such a frame after being dipped will carry a helical surface of soap-film.

Exp. 14.—If, instead of a single helix of wire, two helices are fixed to the same axis, but not quite symmetrically, so that in any part the wire of one helix is nearer that above it than the one below, two helical films will not be formed, but there will be a single one in an intermediate position which will be joined to the two wire screws by a pair of conical screw-surfaces, these forming with the true screw-surface a screw-shaped trough down which bubbles may be rolled or up which they may be wound, as water is wound up by a screw-pump (fig. 13). Further, if a series of small bubbles are blown along the helical edge in which the three films meet at angles of 120°, a spiral staircase is made of soap-film, down which a bubble will run one or two steps at a time, and from which it will escape uninjured when it reaches the bottom. Of course bubbles lighter than air, in the same way, will rest against the lower sides of a trough or roll up instead of down the screw-surface.

Exp. 15.—One more experiment in which the rolling of bubbles is the chief feature is worth describing. Three rings of wire, seen in section in fig. 14, are joined together by wires, shown dotted, and are carried by a central axis, which may be made to rotate. After this frame has been dipped in the solution of soap, and the three radial planes broken, it is found to carry a circular trough, into which a series of bubbles may be dropped, while at the same time the frame may be kept rotating, so that the bubbles are rolling round and round like marbles on the rim of a solitaire-board. A corresponding frame might possibly be made of light wire, which after dipping would rest on the bubbles in the first frame, thus forming a working model of the ball-bearing. I have not, however, succeeded.

Plateau has mentioned the fact (p. 166) that M. Chautard has found a soap-film a convenient envelope for a gas which is to be tested magnetically. He says that a spherical film above one pole of an electromagnet is visibly disturbed if the gas within has magnetic properties when the exciting current from a battery of 25 to 30 Bunsen's cells is made and broken. If, instead of a spherical bubble, one of cylindrical form, with its length about three times its diameter, is used, the distortion produced by a small disturbing force is so greatly magnified that, using an electromagnet actuated by five Grove's cells only, not only is the change of form manifest when oxygen is the gas in the bubble, but it is even possible, by making the length such that the form is very nearly unstable, to cause the bubble to divide the moment the current is made to pass round the electromagnet. With the same means I have not been able to detect any change of form in a spherical bubble. Fig. 15 shows a convenient apparatus for producing, as often as may be desired, a cylindrical bubble of any degree of stability.

The short tube *a* is in connexion with a supply of oxygen which is employed to blow the spherical bubble shown by the dotted circle. According to the position of the screw *d* this bubble will be larger or smaller before it comes in contact with the ring *b*, which is held down by the loose weight *w*. The gas-tap is then immediately turned off, and the ring *b* raised by the action of the weight *c*, until the screw *e* brings the movement to a stop, Thus the length of the cylinder depends on the screw *e*, while its volume is determined by the screw *d*, and so whatever degree of stability is found suitable can be reproduced as often as may be required. The poles of the magnet should be placed at about the level of the line *pp*.

There is one other property of a pair of soap-films resting against one another, but not in contact, to which I have not referred. In a lecture at the Royal Institution a few years ago Lord Raleigh showed that two water-jets if perfectly clean will, if directed so as to meet one another at a small angle, be reflected again and fall as two separate jets, never really coming into contact at all. If the water is not perfectly clean, the experiment will not succeed. He showed that such a pair of mutually reflected jets form a very delicate electroscope, so that if a piece of excited sealing-wax is held even at a considerable distance they instantly coalesce. As the two jets in his experiments and the two bubbles in those which I am about to describe are in each case kept apart by a thin film of air, I expected to find a pair of bubbles attached to two rings in the same way act as a delicate electroscope.

Exp. 16.—If a pair of bubbles are blown on rings, which must be insulated from one another, as shown in fig. 16, and the cover of a small electrophorus is raised even at some yards distance, instantly the two bubbles coalesce as seen in fig. 17, but do not burst as they have hitherto been found to do. Or if the two rings are connected with a key and a single bichromate-cell so that when the key is not pressed the rings are connected together, but when depressed they form the terminals of the cell, then at the moment of making the contact the bubbles unite because the electrostatic attraction between surfaces so very close together is able to squeeze out the air, which mere pressure had hitherto failed to do.

Exp. 17.—Bearing in mind how exceedingly delicate this is as a test of difference of potential, the following experiment seems the more decisive. The cover of the electrophorus may be brought so close to the side of the bubbles, shown in fig. 4, as to pull them completely out of shape, and yet the outer film so completely screens the inner from the electrical action, even though the inner one is to all appearance in contact with the outer one, that there is no difference of potential between them, and so the film of air is not destroyed. I do not know any experiment which so clearly shows as this that electrical force is confined to the absolute surface of a conductor, and is not felt at any depth within it however small.

Plateau has mentioned, p. 168, that a hemispherical film blown on a plate will screen a smaller hemisphere blown within it, and also resting on the plate, from electrical disturbance; but in this case the two films are widely separate, and there is not the same delicate test as in the case of two bubbles apparently coincident, which instantly join when the smallest electrical stress exists between them.

Exp. 18.—One more experiment, which is a combination of these two, is worth performing. If one of the bubbles of fig. 16 is replaced by the combination shown in fig. 4 while the other remains as before, and if the cover of the electrophorus is raised anywhere in the neighbourhood, immediately the two outer films join and become one, while the inner bubble undamaged and the heavy ring slide down to the bottom of the now enlarged single bubble, and give rise to the form shown in fig. 18.

I am perfectly sensible of the fact that these experiments lie very closely on that ill-defined border-line which separates scientific work from scientific play, but I trust that the beautiful way in which they illustrate the action of certain forces may be sufficient excuse for my showing members of the Physical Society what cannot fail to remind some of them of their nursery days.

The following particulars may be of service to those who wish to repeat any of these experiments.

The solution that I have used is composed of

1	part by weight	oleate of soda.
40	„	distilled water.

These, when solution is complete, are well mixed with one third the volume of glycerine and left for a week to settle in stoppered bottles. The liquid is then syphoned off from the impurities which have risen to the surface and clarified with a few drops of ammonia.

The thick wire rings and frames are made of tinned iron wire 1½ millim. in diameter, well cleaned with emery cloth.

The thin wire rings may be made of any thin wire, but aluminium about ½ millim. in diameter does well.

I have found it necessary to make a blowpipe with a trap as shown in fig. 19 to catch condensed moisture, which is apt to cause a failure if it mixes with the bubble. The diameter of the mouth at *a* is 7 millim. For detaching small light bubbles a pipe with a smaller mouth should be used.

When both gas and air are used in any experiment and it is necessary to regulate the proportions very carefully it is well to have a T-piece attached to the blowpipe, so that either gas or air may be blown or stopped at pleasure.

XXVII. *On the Use of the term "Resistance" in the Description of Physical Phenomena.* *By* R. H. M. Bosanquet[*].

The following observations were suggested by a remark quoted by Prof. S. P. Thompson from Dr. Hopkinson, at the meeting of the Physical Society on January 28, 1888. The remark was to the effect that there is no such thing as magnetic resistance, because the resistance of iron to magnetization is not constant under varying conditions. I wish to bring before you the question, What is the fundamental idea involved in the use of the term "resistance" in the description of physical phenomena?

If we apply force to produce any change, say to extend an extensible body, or compress a compressible one, the body is said to offer resistance to the force. If we have a second body which requires a greater force than the first to produce the same change, we say that the second body offers a greater resistance than the first. Consequently resistance increases with the force employed to produce a given change.

Again, if in the second body the same force produces a greater change than in the first, the resistance of the second body is said to be less. Consequently resistance diminishes as the change produced increases, and *vice versâ.*

The change may consist in the flow of a fluid, through a tube say, under pressure; then we should say that the resistance offered by the tube to the flow increases as the pressure increases and as the flow diminishes.

Or, again, we may consider a liquid being evaporated under the action of heat. We may say that the resistance to evaporation is the greater the more heat is required and the less liquid evaporated.

We may shortly refer to the two elements in any such change as the cause and the effect. And we shall generalize the conception of resistance in stating that it is the greater the greater the measure of the cause and the less the measure of the effect. But nothing is so far assumed as to the precise law which connects these quantities.

If we now refer to the case of electrical resistance, we have a law suggested by Ohm's law, viz., that the measure of the resistance is the direct ratio of the measures of cause and effect. Or, to take the words "measure of" as understood, resistance is the ratio of cause to effect, or the quotient of cause by effect; or, resistance is proportional to the cause and inversely as the effect.

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[*] Read February 11, 1888.

This is not the least necessary. We might very well, if there were any reason for it in any particular case, adopt as the measure of resistance such a function as the square, or the square root of the above ratio. It would be quite compatible with the general definition deduced above from the meaning of the word. But Ohm's law ties us down to the direct ratio.

If, therefore, we extend the analogy of Ohm's law to other cases, we shall have some such propositions as follows:—

If force be employed to extend an extensible body or to compress a compressible one, the resistance to extension or compression, on the analogy of Ohm's law, is the quotient of the cause, or force employed, by the effect, or amount of change.

If liquid flow through a pipe under a difference of pressure between its ends, the resistance, on the analogy of Ohm's law, is the quotient of the cause, or difference of pressure, by the effect, or rate of flow.

In such cases the precise manner in which cause and effect are measured must be determined by convenience. It does not in the least affect the conception of resistance as the ratio of cause and effect.

Again, if a liquid be evaporated under the action of heat, the resistance to evaporation, on the analogy of Ohm's law, is the ratio of the cause, or heat expended, to the effect, or amount of liquid evaporated, no matter how these are measured.

We may add a further illustration. Suppose that any substance is heated. Different substances will rise to different temperatures in virtue of their different specific heats. Here the resistance to heating up, on the analogy of Ohm's law, is the ratio of the cause, or heat expended, to the effect, or rise of temperature produced, no matter how these are measured.

In all these cases we have a perfectly intelligible physical conception of resistance, strictly in accordance with the natural meaning of the word in the English language.

But I think we should not apply by way of objection in such cases arguments such as the following :—*i. e.* (1) That the resistance so measured is not of the same dimensions as electrical resistance ; or (2) that the resistance so measured is not a constant, but liable to variation depending on the conditions, as well as to complication by other factors.

The ground that underlies the employment which has been made of these two arguments in opposition to the use of the term resistance in connexion with magnetism, is really the assumption that the same word ought to imply identity in the two cases of electricity and magnetism, and not analogy. If it be admitted that analogy is sufficient ground for the use of a word, the objections disappear.

But if used in connexion with two different subjects, it is quite impossible that the meanings of the word can be identical. And that there is no objection in practice to the use of the same word in meanings justified by analogy only may be shown by many illustrations. Take the case of the word Potential. It originated I suppose in connexion with mechanical theory. It has a definite mechanical meaning, and definite dimensions regarded as a mechanical quantity. But we extend the term without difficulty to both the magnetic and electrical analogies ; and in each of these cases it has another definite meaning, and other definite dimensions. Yet nobody thinks of objecting to either of these uses of the term potential, because they involve definitions of the dimensions of potential, different both from that used in mechanics and from each other.

Similarly with the word Force. This is a purely mechanical term. But nobody objects to its use in the terms electromotive force, or magnetizing force because these uses involve definitions of the dimensions of Force different both from that used in mechanics and from each other.

It is fully understood in these cases that analogy is all that is involved, and nobody supposes that there is any question of identity.

I will now for a moment compare the case of magnetic resistance with a natural case of ordinary resistance, to show that the analogy between the two is extremely close.

Consider a mass of matter in any condition; suppose it enclosed in a vessel and subjected to compression. Then the cause will be the pressure, the effect the compression, and the resistance on the analogy of Ohm's law the ratio of pressure to compression. If we choose to measure the compression by the inverse of the volume, the resistance will be the product of pressure and volume.

If we suppose the matter to be at first gaseous, and the compression to take place with loss of heat according to Boyle and Marriotte's law, the resistance will be so far constant. But as the compression advances, and the liquid and solid states are approached, the resistance will necessarily increase, and when the compression reaches a certain value it will be practically incapable of proceeding further, and the resistance will increase indefinitely with the pressure. The resistance is therefore here a function of the compression, *i. e.* of the effect, just as magnetic resistance is a function of magnetic induction; and, if we omit the initial part of the curves of magnetic resistance of bars, the rest of them is oddly similar to the course of the resistance in the case imagined.

Now the resistance in the case imagined (of the compression of matter) is not constant, but is a function of the effect just as magnetic resistance is. Is this want of constancy an objection to the use of the term resistance as a description of that quality in matter which tends to prevent compression? For if it is a sufficient objection to the term magnetic resistance, it must be a sufficient objection to the term resistance to compression as well, as the analogy between the two cases is so extraordinarily close. It cannot be maintained for a moment that the want of constancy offers any objection.

An objection may be taken possibly that the magnetic resistance has varying values for rising and falling magnetizing force, and is therefore not definitely ascertainable as a function of magnetizing force. Answer: We do not give up trying to find out the true values of things because they are superficially complicated with others. If magnetic retentiveness does lead to complications, it is our business to disentangle them; and practically the method of reversals of magnetizing current enables us to do this. But it is not maintained, at least by me, that magnetic resistance is properly a function of magnetizing force. It is a function of the effect produced, *i. e.* of the magnetic induction, as is amply proved by the inspection of the numerous curves that have been published in connexion with this question. And it is so just in the same way in which the resistance of matter to compression is a function only of the compression, or of the stage which the condensation has reached.

A word may be usefully said here on the origin of the differential formula used by Lamont to express the so-called magnetic conductivity of a magnet. The ambiguity on which the origin of this formula depends can be well dealt with by the present illustration.

If we consider the compression of matter which has attained the liquid condition, we may speak of the resistance to any further compression at this point, or under these conditions, as being infinite. For the effect produced by the further pressure is nothing. But in the magnetic analogy we always consider a change, the initial condition of which corresponds roughly to the gaseous condition, or rather a pregaseous condition, in the material analogy. In either case, if, instead of considering the total change, we consider the state of things at a point, then we must suppose the cause to vary by a small quantity, d cause, producing a small effect, d effect; and the ratio of these may be said to be the resistance at the point of the representative curve, or under the given conditions. It is on this mode of statement that Lamont's differential formula is based. The formula is (*Handbuch des Magnetismus*, p. 41),

$$dm = k\,(\mathrm{M}-m)\,dx\,;$$

whence by integration,

$$\mathrm{M}-m = \mathrm{C}e^{-kx}\,;$$

x = magnetizing force,
m = magnetism,
M = maximum of magnetism.

Here dm/dx is called the magnetic conductivity; and it is inversely as the *magnetic resistance at a point.* That magnetic conductivity, which is inversely as the magnetic resistance as I and others use the term, is m/x, i. e. has reference to the *total change.* I have shown (Electrician, xvi. p. 247) that the assumption $m/x = k\,(M - m)$ is that to which we are thus led, in place of Lamont's formula above given. Prof. S. P. Thompson has shown that this formula, which is the same as that known as Frölich's, corresponds with the facts better than Lamont's.

In what precedes I have, for convenience in quotation, used the ordinary term magnetizing force, and regarded it as a cause. As, however, so-called magnetizing force is identical in dimensions and physical nature with the rest of the magnetic induction which it develops, I use the expression in this way under protest only; for I regard it as inconceivable that cause and effect should be of identical nature, unless the effect reacts again as a cause, so that the smallest original cause drives the effect up to saturation. Without going into detail I may mention an analogy where cause and effect are identical in nature, and the process does therefore necessarily always go to saturation; viz., the case of the multiplication of germs, in a habitat of limited capacity. Here the individuals which formed the original cause multiply; the progeny constitute at once the effect and an increase of the cause. And the smallest original infection of the cause is enough to develop saturation, *i. e.* the highest population that the habitat can maintain. Now in the case of magnetism, magnetizing force and the rest of the magnetic induction are by definition identical in nature. If they are cause and effect, the effect should therefore act again as cause, so as to produce saturation from the smallest force; but this does not occur. I prefer therefore to regard the difference of potential as the cause, as is always done in the case of the electric current.

There is only one further point. It may be maintained that the mention of Ohm's law involves *sub silentio* the assumption that the corresponding law holds true, *i. e.* that the resistance is constant. If this is supposed, it is only necessary to explain that it is not intended. It is clearly legitimate to measure a quantity in the manner suggested by Ohm's law (*i. e.* by the ratio of cause to effect), and speak of the laws or courses of values thus obtained as representing in the different cases the analogues of Ohm's law. These are the laws which occupy the place in the various matters dealt with, which Ohm's law occupies in the subject of the flow of electricity, *i.e.* they express the ratio of cause to effect in the different cases. But the statement that they are the analogues of Ohm's law does not involve the position that they are identical with it, any more than the definitions of Potential or Force in electricity and magnetism are identical with those in mechanics or with each other.

The main points I have dealt with are:—That resistance may be conceived of quite generally as the ratio of cause to effect.

That the objections to the employment of the term magnetic resistance have been founded on the assumed requirement of identity instead of analogy between the affections of different subjects, and cannot be sustained.

And that the extension of the employment of the term Resistance in the above manner leads to some remarkable analogies which go far to justify independently the employment of the term Magnetic Resistance. And it has been pointed out that the ordinary mode of statement involves inconsistent and impossible ideas as to the relation of cause and effect in the phenomena, whereas the application of the term Magnetic Resistance compels us to precisely ascertain these, and put them in their right places.

Note.—An objection has been recently raised by M. Hospitalier* to my definition of Magnetic Resistance, which comes to this, that in defining the effect I take the induction through unit surface instead of the total induction. This objection is again based on the supposed necessity for conforming identically to Ohm's law in Electricity. The foregoing remarks will have made it clear that I consider this unnecessary, the question being, What is the most convenient measure of the effect? Since the magnetic potential attainable is practically limited, though it increases with the dimension, and the induction through unit of area practically attainable is also limited (by saturation), the ratio of these two quantities lies always within certain limits, though it increases with the dimensions.

* 'Electrician,' xx. p. 164, note.

In fact the magnetic resistance thus estimated is of linear dimension, and may be regarded as of linear scale in plans. If, therefore, this quantity be selected to be tabulated, the discussion of questions of design from a practical point of view is greatly facilitated, and the relations of the quantities involved are more easily followed and more simply expressed.

XXVIII. *The Efficiency of Incandescent Lamps with Direct and Alternating Currents. By* W. E. AYRTON *and* JOHN PERRY[*].

IT is now well understood that in order to economically distribute power by means of electricity it is necessary to employ a high potential-difference, or P.D., between the mains and a small current flowing through them, while considerations of safety require that the P.D. between the leads in the houses shall not exceed 100 or 200 volts. Hence some system of converting a large P.D. and small current into a small P.D. and large current has to be employed; and the four systems of conversion that have hitherto been devised consist in the employment of:—1. Motor-Dynamos, 2. Accumulators, 3. Alternating Current-Transformers, 4. Direct Current-Transformers.

Of these four methods the third is the one that is most extensively utilized at the present time; indeed it is the only system of conversion that is at all extensively used, at any rate in this country. But in view of some direct current system of conversion also coming into common use, there arises a question of considerable importance to the consumer, viz. Is more light obtained with the same expenditure of power with direct or with alternating currents? And apart from considerations of the electric distribution of power on a large scale, the answer to this question is of importance in supplying one factor towards the decision as to the relative advantages and disadvantages of direct and alternating currents for detached installations.

Where the electric energy supplied to a consumer has been charged by meter, people have, as a rule, been content to measure only the number of coulombs supplied, ignoring altogether any variation in the volts; but as electricity *per se* apart from the P.D. is of no commercial value whatever, and therefore is unlike water apart from pressure, it is clear that in estimating the value of a supply of electric energy for lighting purposes we must measure the watts and not merely the current. The problem, therefore, that we have attempted to solve is whether a Board of Trade unit (1000 watt-hours) is more valuable for lighting by incandescent lamps when the current is direct or when the current is alternating.

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[*] Read February 25, 1888.

For a complete solution of this problem we must ascertain not merely the "efficiency of the lamp" or the candles per watt with the two systems of supply, but also the life of the lamp, since the cost of lamp renewals may be as important a question for the consumer as the bill for electric power. Unfortunately, however, there does not exist, as far as we are aware, any accurate information as to the life of an incandescent lamp with various alternating P.Ds.; indeed the data at our disposal for the life of a lamp with various non-alternating P.Ds. is at most meagre. And if any Member of the Society can supply us with information regarding the life of some fixed type of incandescent lamp either with different direct P.Ds. or with different alternating P.Ds. we shall be grateful for the information, as it will furnish us with a further opportunity of using the method described in our paper read before this Society for deciding on "The most Economical Potential Difference to employ with Incandescent Lamps."*

For the present, therefore, we shall confine ourselves solely to the question of efficiency, and, as it is known that the efficiency of an incandescent lamp increases with the current passing through the lamp, it is clear that to obtain an accurate comparison of the efficiencies with direct and alternating currents, we must employ exactly the same current in both cases, or rather the same mean square of the current, for this will be most likely to develop the same rate of production of heat in the lamp, since this rate is proportional to the resistance into the mean square of the current, whether the current be direct or alternating, and whether or not there be self-induction in the circuit in which the heat is developed. We say will be most likely to develop the same rate of production of heat, since we must not assume without proof that for the same mean square of current the resistance of a carbon filament is the same whether the current be direct or alternating.

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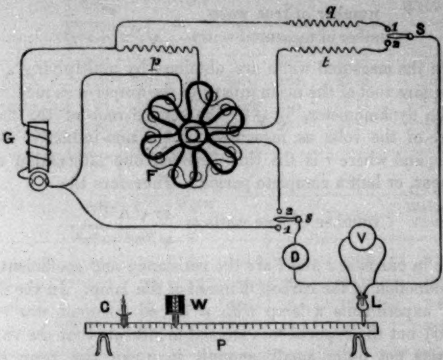
* Proc. Phys. Soc. vol. vii. p. 40.

Some writers have stated that the current, as measured by an electrodynamometer, required to be sent through an incandescent lamp when emitting a definite amount of light, had a different value when the current was an alternating one from what it had when the current was direct. This difference might be due either to a defect in the dynamometer measurement or to some variation in the light standard. If the wire with which a dynamometer is wound be thick, then the current-density may be far from uniform when the current is alternating; and on this account, as observed by Captain Cardew some years ago, a dynamometer might give different readings for different rates of alternations of the current, while an incandescent lamp in circuit with it remained equally bright. To avoid this possible cause of error the dynamometer, which was constructed by Messrs. Shepherd, Vignoles, and Wheatley (the three of the students of the Central Institution who carried out the investigation), was wound with much finer wire than would usually be employed in the construction of a dynamometer not required to read currents much below one ampere. The dynamometer was for that reason unnecessarily sensitive, and it required a fairly strong spring to control the motion of the suspended coil. This led to an unnecessary waste of energy in the dynamometer, but that was of no consequence in this investigation, as our object was to measure the mean square of the current most accurately, and not to satisfy the condition, which is of considerable importance in the design of commercial measuring-instruments, of wasting as little energy as possible in the instruments.

To the suspended coil of the dynamometer was attached a mirror, and the values of the deflections of the spot of light were determined by direct comparison with the simultaneous readings of an accurately calibrated magnifying spring-ammeter when various direct currents were successively sent through the circuit. The sensibility was such that a deflection of 400 scale-divisions was produced on a scale 68·88 inches away, for a current of 2·53 amperes, which corresponds with a deflection of 140 scale-divisions for 1·5 ampere,



and this was about the usual current passing through the dynamometer in the actual lamp experiments. The value of the current could, therefore, be accurately measured by this dynamometer, which is indicated by D in the figure, and by knowing what fraction of the current passing through the dynamometer D passed through the non-inductive voltmeter V, the current passing through the lamp is known.

Any error that might have arisen from a variation of the light-standard was eliminated by taking successive readings with a direct current produced by a Gramme-dynamo G, and with an alternating current produced by a Ferranti-dynamo F, the switches S and *s* being turned to 1, 1 in the first case and to 2, 2 in the second. The Gramme-dynamo G was also used to excite the field magnets of the Ferranti, a suitable current being obtained by a proper adjustment of the resistance *p*. By means of the resistances *q* and *t* the direct and alternating currents passing through the incandescent lamp L could be respectively varied; and it was found that if these resistances were so adjusted that the reading of the dynamometer D was the same in both cases, so also was the reading of the non-inductive voltmeter V.

It is known when power is supplied by means of an alternating current to a circuit of resistance r ohms and coefficient of self-induction l secohms that

$$\frac{\text{number of true watts}}{\text{number of measured watts}} = \frac{r\tau}{\sqrt{l^2\pi^2 + r^2\tau^2}},$$

where the measured watts are obtained by multiplying $\sqrt{\overline{A^2}}$, the square root of the mean square of the amperes as measured by the dynamometer, by $\sqrt{\overline{V^2}}$, the square root of the mean square of the volts as measured by the non-inductive voltmeter, and where τ is the time between one alternation and the next, or half a complete period. Therefore the

$$\text{number of true watts} = \frac{r\tau\sqrt{\overline{A^2}\,\overline{V^2}}}{\sqrt{l^2\pi^2 + r^2\tau^2}},$$

where in our case r and l are the resistance and coefficient of self-induction of the carbon filament of the lamp. In the first set of experiments a lamp with a looped filament was employed; but the experiments seemed to indicate that the value of l was not quite small enough to make the term $l^2\pi^2$ absolutely negligible, or else that there was some slight mutual induction between the dynamometer-coils and a small brass vessel containing oil, in which moved a damping-vane attached to the moving coil. The results were therefore discarded, and this vessel was replaced by one made of non-conducting material, and all metal near the dynamometer was as far as possible removed. The lamps subsequently employed were first one with a M-shaped filament, and another with a simple horseshoe-shaped filament, each possessing little self-induction.

The light was measured by comparison with a standard candle, the two being placed 130 centimetres apart on the photometer P, and a screen composed of two pieces of paraffin-wax W, with silvered paper between them, was adjusted until the two pieces of wax appeared equally bright, the comparison being first made when the screen was looked at through ruby-red glass and then through signal-green glass.

The following is a sample of the results obtained, a and b being the distances respectively of the screen from the incandescent lamp and from the standard candle.

TABLE I.

	$\sqrt{\overline{A^2}}$.	$\sqrt{\overline{V^2}}$.	Watts.	Green.			
				a.	*b.*	Candles.	Watts per Candle.
Ferranti......	1·34	50	67	107·7	22·3	23·33	2·872
Gramme......	1·34	50	67	105·8	22·2	23·52	2·848
Ferranti......	1·34	50	67	107·9	22·1	23·74	2·823
Gramme......	1·34	50	67	108	22	24·11	2·779

	$\sqrt{\overline{A^2}}$.	$\sqrt{\overline{V^2}}$.	Watts.	Red.			
				a.	*b.*	Candles.	Watts per Candle.
Ferranti......	1·34	50	67	106·5	23·5	20·52	3·265
Gramme......	1·34	50	67	106·4	23·6	20·34	3·295
Ferranti......	1·34	50	67	106	24	19·51	3·435
Gramme......	1·34	50	67	105·9	24·1	19·32	3·469

It will be observed that in the set of four successive observations given in Table I. for green light, the candle-power of the lamp appears to be steadily increasing, a result probably due to the brightness of the candle slowly diminishing; but although this would make the *absolute* determination of the efficiency of the incandescent lamp inexact, it introduces no error in the determination of the *relative* efficiencies of the lamp with direct and alternating currents, since every observation with the direct current was immediately followed by one with alternating current, so that the means of all the corresponding direct-current measurements may be safely compared with the means of all the alternating-current measurements. Similarly for this investigation it is unimportant whether the standard candle used on one day was slightly more or less bright than the standard candle used on the following day. Further, since for any two successive observations with the same coloured light with direct and with alternating current the screen was at practically the same distance from the lamp, the fact that the rays of light coming from the lamp made a somewhat different angle with the surface of the paraffin-wax from the angle made by the rays coming from the candle, introduced no error in this investigation of *comparative* efficiencies, although it might very likely do so in an absolute determination of the efficiency of an incandescent lamp. In fact the simple device of successively taking observations with direct and with alternating currents throughout the whole investigation removed most of the objections that usually may be made against photometric determinations carried out with a candle as the standard of light.

Disregarding the experiments made with the lamp with the looped filament, for the reason given above, Table II. gives the summary of the results obtained; and the conclusion to be drawn from them is that although the watts per candle for green light are not quite the same for direct and for alternating currents, and although the volts per candle for red light are also not exactly the same for direct and alternating, still the difference between the results for the same colour is so small that it may be put down to experimental errors; and this, combined with the fact that the mean of all the 75 experiments gives practically the same number of watts per candle for both direct and alternating currents, leads to the practical certainty that *the efficiency of an incandescent lamp is the same for both direct and alternating currents.*

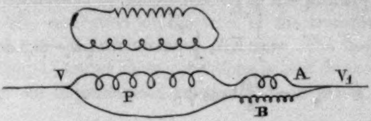
TABLE II.—Mean Values.

<table>
<tr><th>Lamp.</th><th>No. of Experiments made.</th><th>$\sqrt{\overline{A^2}}$.</th><th>$\sqrt{\overline{V^2}}$</th><th colspan="4">Watts per Candle.
White Light.</th><th>Time of an alternation, in seconds.</th></tr>
<tr><td>50 volt M-shaped filament.</td><td>20</td><td>1·25</td><td>50·5</td><td colspan="2">Gramme.
3·053</td><td colspan="2">Ferranti.
3·033</td><td>$\frac{1}{453}$</td></tr>
<tr><td></td><td></td><td></td><td></td><td colspan="2">Green Light.</td><td colspan="2">Red Light.</td><td></td></tr>
<tr><td></td><td></td><td></td><td></td><td>Gramme.</td><td>Ferranti.</td><td>Gramme.</td><td>Ferranti.</td><td></td></tr>
<tr><td>50 volt horseshoe-shaped filament</td><td>19</td><td>1·30</td><td>50·0</td><td>2·597</td><td>2·534</td><td>3·100</td><td>3·100</td><td>$\frac{1}{453}$</td></tr>
<tr><td>50 volt horseshoe-shaped filament</td><td>20</td><td>1·34</td><td>50·0</td><td>2·935</td><td>2·966</td><td>3·254</td><td>3·164</td><td>$\frac{1}{167}$</td></tr>
<tr><td>50 volt horseshoe-shaped filament</td><td>16</td><td>1·35</td><td>50·0</td><td>2·900</td><td>3·073</td><td>3·504</td><td>3·477</td><td></td></tr>
<tr><td colspan="4">Mean of the last three results ....</td><td>2·811</td><td>2·857</td><td>3·286</td><td>3·247</td><td></td></tr>
<tr><td colspan="4">Mean of all the 75 experiments ....</td><td colspan="2">Gramme.
3·0490</td><td colspan="2">Ferranti.
3·0497</td><td></td></tr>
</table>

In all the experiments the plane of the filament was perpendicular to the screen.

XXIX. *On the Measurement of the Power supplied to the Primary Coil of a Transformer.* *By* E. C. Rimington*.

In the discussion on Mr. Kapp's paper on Transformers, at the Society of Telegraph Engineers, Professor Ayrton gave a formula for calculating the true power supplied to the primary from the reading of a Siemens wattmeter. The thick wire coil of the wattmeter is in series with the primary coil, and the fine wire coil connected as a shunt on the two, as in the diagram, where P is the primary coil, A the thick and B the fine wire coil of the wattmeter. Let r_1 be the resistance of the primary P including A, and L_1 its coefficient of self-induction also including A; let r_2 and L_2 be the resistance and coefficient of self-induction of B; i_1 and i_2 the currents in P and B respectively. Let e be the potential-difference between the points V and V_1. Now



$$e = E \sin at, \text{ where } a = \frac{2\pi}{T},$$

T being the periodic time, and E = the maximum value of e. Also

$$i_1 = A_1 \sin(at - \psi_1), \text{ where } \tan\psi_1 = \frac{aL_1}{r_1},$$

and

$$i_2 = A_2 \sin(at - \psi_2), \text{ where } \tan\psi_2 = \frac{aL_2}{r_2}.$$

Let δ be the reading on the torsion-head; then

$$\delta = \frac{k}{T}\int_0^T i_1 i_2\, dt\,; \; k \text{ being some constant.}$$

$$\delta = \frac{k}{T} A_1 A_2 \int_0^T \sin(at - \psi_1)\sin(at - \psi_2)\, dt,$$

$$= \frac{kA_1A_2}{2T}\int_0^T \{\cos(\psi_2 - \psi_1) - \cos(2at - \psi_1 - \psi_2)\}\, dt,$$

$$= \frac{kA_1A_2}{2}\cos(\psi_1 - \psi_2).$$

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* Read March 10, 1888.

Now $$A_2=\frac{E}{S}, \text{ where } S=\sqrt{r_2^2+a^2L_2^2}.$$

Hence
$$\therefore\delta=\frac{k}{S}\cdot\frac{EA_1}{2}\cos(\psi_1-\psi_2).$$

Now mean power, or
$$\begin{aligned}p_m&=\frac{1}{T}\int_0^T ei_1dt,\\ &=\frac{1}{T}\,EA_1\int_0^T\sin at\,.\,\sin(at-\psi_1)\,dt\\ &=\frac{EA_1}{2}\cos\psi_1.\end{aligned}$$

Therefore
$$p_m=\frac{\delta}{k}\,.\,S\,.\,\frac{\cos\psi_1}{\cos(\psi_1-\psi_2)}.$$

But
$$\begin{aligned}\frac{\cos\psi_1}{\cos(\psi_1-\psi_2)}&=\frac{\cos\psi_1}{\cos\psi_1\cos\psi_2+\sin\psi_1\sin\psi_2}\\ &=\frac{\dfrac{1}{\cos\psi_2}}{1+\tan\psi_1\tan\psi_2},\end{aligned}$$

$$\begin{aligned}\frac{\cos\psi_1}{\cos(\psi_1-\psi_2)}&=\frac{\sqrt{1+\tan^2\psi_2}}{1+\tan\psi_1\tan\psi_2}=\frac{\sqrt{1+\dfrac{a^2L_2^2}{r_2^2}}}{1+\dfrac{a^2L_1L_2}{r_1r_2}}\\ &=\frac{r_1S}{r_1r_2+a^2L_1L_2}.\end{aligned}$$

Hence
$${}_m=\frac{\delta}{k}\;\frac{r_1S^2}{r_1r_2+a^2L_1L_2}.$$

Now for permanent currents,
$$\delta=k\frac{e}{r_2}C, \text{ and } eC=K\delta,$$

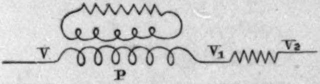
where K is the constant of the instrument for watts.

Therefore
$$k=\frac{r_2}{K},$$

or
$$p_m=K\delta\,.\,\frac{r_1(r_2^2+a^2L_2^2)}{r_2(r_1r_2+a^2L_1L_2)}.$$

In order to be able to make use of this result it is necessary to know the coefficients of self-induction of P + A and B; the latter may be found once for all by any of the well-known methods and its value marked on the instrument; but the former will require to be determined for the same values of the currents in the primary and secondary coils of the transformer as are flowing when the power is measured, since the apparent coefficient of self-induction of the primary coil depends on the saturation of the iron of the transformer and also on the current in the secondary coil. The best method of measuring L_1 under these conditions is that due to Joubert.

Connect an inductionless resistance R in series with the primary, and pass an alternating current of known period through the two; arrange the resistances of the primary and secondary circuits so that the currents in them have about the same values as when the power-measurement was made. Now connect a high-resistance Siemens electrodynamometer* between V and V_1, and let the reading be δ_1; again connect it between V_1 and V_2 and let the reading be δ_2.



Then

$$\frac{\sqrt{r_1^2 + a^2 L_1^2}}{R} = \sqrt{\frac{\delta_1}{\delta_2}},$$

$$L_1 = \frac{1}{a}\sqrt{R^2 \frac{\delta_1}{\delta_2} - r_1^2}.$$

This method of calculating the mean power supplied to the primary of a transformer is, however, inconvenient, as it entails the use of a high-resistance dynamometer, with which two observations must be taken to obtain L_1, in addition to the trouble of adjusting the resistances of the two circuits to obtain the same conditions as when the wattmeter-reading was taken.

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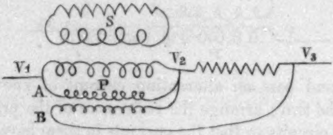
* A Cardew voltmeter may be employed, in which case

$$L_1 = \frac{1}{a}\sqrt{R^2 \frac{\delta_1^2}{\delta_2^2} - r_1^2}.$$

The following method, in which a high-resistance electrodynamometer is employed, enables the power given to the primary coil to be measured from one reading without the knowledge of either the resistance or the coefficient of self-induction of the primary.

Let the two coils of the electrodynamometer be A and B, and let their resistances and coefficients of self-induction be $r_1 r_2$ and $l_1 l_2$ respectively; moreover, let $\frac{l_1}{r_1}=\frac{l_2}{r_2}$, that is to say, let the time-constants of the two coils be the same; this can be easily effected by putting an inductionless resistance in series with one or other of the coils. Connect as in the diagram.



Let the primary coil be put in series with an inductionless resistance R. Let the potential-difference between V_1 and V_2 $=E_1 \sin at$; then i, the current through the primary coil and through $R=\frac{E_2}{R}\sin(at-\psi)$, where E_2 is the maximum potential-difference between V_2 and V_3.

Let ψ_1 and ψ_2 be the angles of lag of the coils A and B respectively, and let i_1 and i_2 be the currents through them at some instant t.

Then, if δ be the reading of the torsion-head,

$$\begin{aligned}\delta&=\frac{k}{T}\int_0^T i_1 i_2\,dt,\\&=\frac{k}{T}\cdot\frac{E_1E_2}{S_1S_2}\int_0^T \sin(at-\psi_1)\sin(at-\psi-\psi_2)\,dt,\\&=\frac{k}{2}\cdot\frac{E_1E_2}{S_1S_2}\cos(\psi_1-\psi_2-\psi);\end{aligned}$$

where

$$S_1=\sqrt{r_1^2+a^2l_1^2} \quad\text{and}\quad S_2=\sqrt{r_2^2+a^2l_2^2}.$$

Now

$$\tan\psi_1=\frac{al_1}{r_1}, \quad\text{and}\quad \tan\psi_2=\frac{al_2}{r_2}.$$

Hence
$$\tan\psi_1=\tan\psi_2,\ \text{since}\ \frac{l_1}{r_1}=\frac{l_2}{r_2},$$
or
$$\psi_1-\psi_2=0.$$
Therefore
$$\delta=\frac{k}{2}\cdot\frac{E_1E_2}{S_1S_2}\cos\psi.$$
Now mean power given to the primary coil, or
$$p_m=\frac{E_1E_2}{2R}\cos\psi,$$
$$=\frac{\delta}{k}\cdot\frac{S_1S_2}{R}.$$
For permanent currents,
$$\delta=k\frac{E_1}{r_1}\cdot\frac{E_2}{r_2}=kE_1C\cdot\frac{R}{r_1r_2}.$$
Also
$$E_1C=\frac{K}{R}\delta,$$
where $\frac{K}{R}$ is the constant for watts.

Hence
$$k=\frac{r_1r_2}{K}.$$
Therefore
$$p_m=\frac{K}{R}\cdot\delta\cdot\frac{S_1S_2}{r_1r_2}.$$
S_1S_2 may be written
$$r_1r_2\sqrt{\left(1+\frac{a^2l_1^2}{r_1^2}\right)\left(1+\frac{a^2l_2^2}{r_2^2}\right)}=r_1r_2(1+\tan^2\psi_1).$$
Hence
$$p_m=\frac{K}{R}\cdot\delta\cdot(1+\tan^2\psi_1).$$
In order that the non-inductive resistance R shall not absorb too much power, B should be the movable coil of the electrodynamometer, as this is generally the one of lower resistance; $\frac{E_1}{S_1}$ should have about the same value as $\frac{E_2}{S_2}$, or
$$\frac{E_2}{E_1}=\frac{S_2}{S_1}\ \text{approximately}.$$
Now
$$\frac{S_2}{S_1}=\frac{r_2\sqrt{1+\tan^2\psi_2}}{r_1\sqrt{1+\tan^2\psi_1}}=\frac{r_2}{r_1};$$
that is, if r_2 is considerably lower than r_1, E_2 will be in the same proportion lower than E_1, and hence R may be a good deal smaller than the impedance of the primary.

XXX. *The Magnetic Circuit of Dynamo Machines.*

By W. E. Ayrton *and* John Perry.*

[Plate VIII.]

In this paper we shall use the following symbols:—Dimensions are given in centimetres, current in amperes, potential differences and electromotive forces in volts, resistances in ohms.

We shall speak indifferently of the drum or Hefner-Alteneck and the cylindric Gramme armature—that is, the Gramme armature which receives its induction through its cylindric surface from two pole-pieces. It is easy to make the slight changes in the formulæ required for flat gramme ring-armatures like those of the Victoria, Gulcher, and Schückert machines, which receive induction through their flat sides, and for all machines with more than two pole-pieces.

r, outside radius of armature.

L, length of armature parallel to the axis.

k, a constant such that $krL=a_1$; k is as much as 1·25 in drum or Hefner-Alteneck armatures, and as little as 0·5 in short cylindric or other Gramme armatures.

t, the thickness of the winding of the armature.

d, the clearance between wire and pole-piece.

$\delta=d+t$, the total distance from iron of armature to pole-piece.

n, the revolutions per second of the armature.

v, the circumferential velocity of the armature in centimetres per second $v=2\pi rn$.

S, wires counting all round the outside of the armature.

A, amperes in each wire.

SA will be called the "ampere-wires" on the armature, being the ampere-turns if it is a Gramme, and being twice the ampere-turns if it is a Hefner-Alteneck.

α, amperes per sq. centim. flowing in the section of the armature-winding made by a plane at right angles to the axis.

α_1, the highest permanent value of α allowable.

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* Read March 10, 1888.

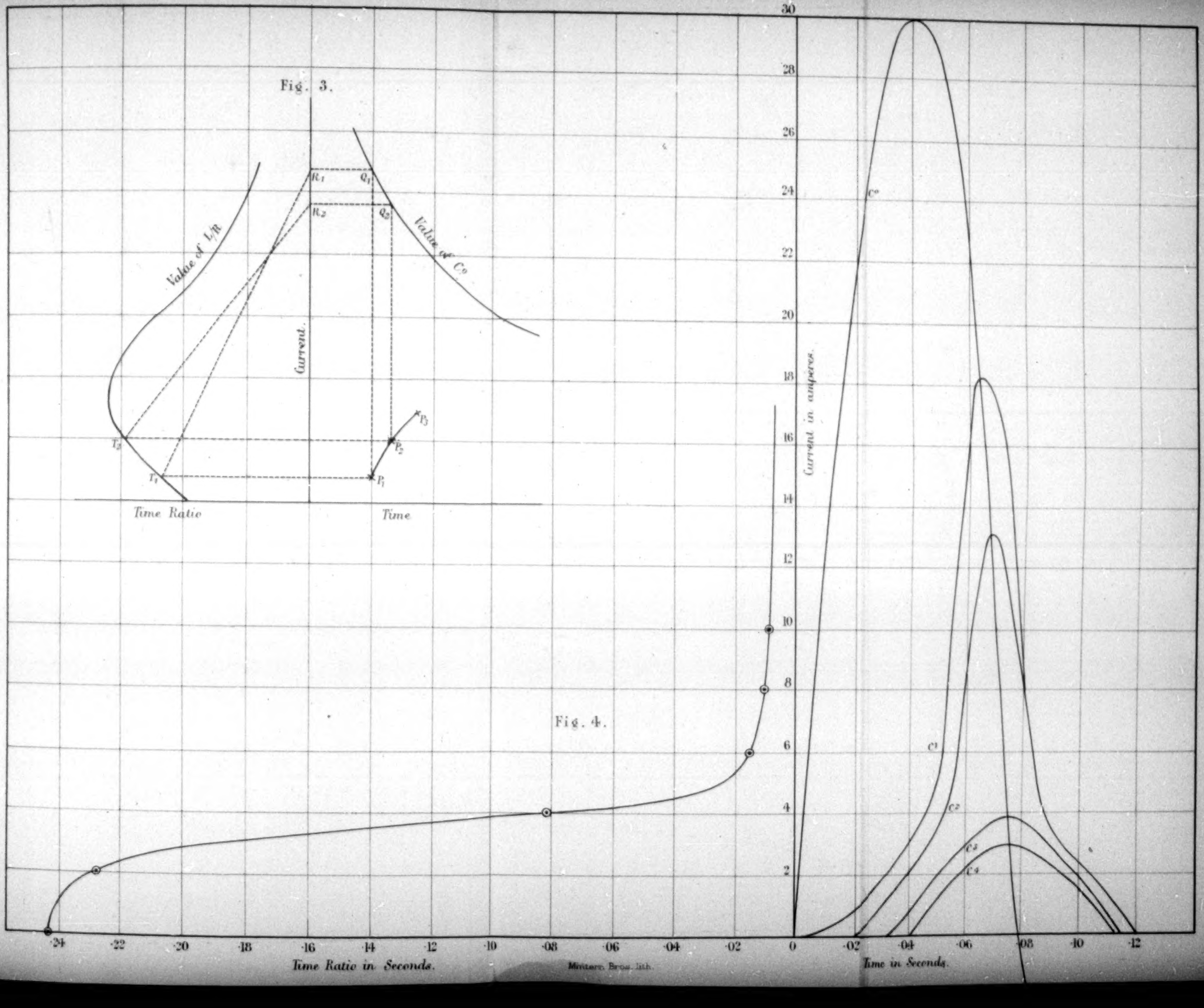
Fig. 3.

Fig. 4.

Mintern Bros. lith.

ρ, the electric resistance of the armature-winding per cubic centimetre. That is, per centimetre of its length per sq. cm. of cross section. Of course ρ is greater than the specific resistance of copper, as the space is partly occupied by insulating material, and ρ is greater with finer wire; but for some practical purposes it is advisable to assume ρ constant for all kinds of winding.

$\rho\alpha^2$, watts developed as heat per cubic centimetre of the winding, so that total rate of development of heat $=2\pi rt\mathrm{L}\rho\alpha^2$.

a_1, cross section of iron of armature by a diametral plane.

a_2, area of pole-piece exposed to the armature, increased by a fringe of $0{\cdot}8\ \delta$ in breadth all round.

a, the cross section of any other portion of the magnetic circuit which may be considered.

β, the induction in C.G.S. units, given in lines per square centimetre anywhere in the magnetic circuit.

β_1, the value of β in the iron of the armature.

β_1', the greatest value of β which it is convenient to employ in the armature when the machine is giving its greatest permanent output.

N, the total induction in the iron of the armature $\mathrm{N}=\beta_1 a_1$ or $\beta_1\ kr\mathrm{L}$.

$\frac{2\delta}{a_2}$ or $\frac{2\,(t+d)}{a_2}$ will be called the magnetic air-resistance, a not very incorrect or misleading expression.

$\frac{l\nu}{a\mu}$ will be called the magnetic resistance of a portion of the iron part of the magnetic circuit whose length is l, whose cross section is a, the permeability being μ, Nν being the total induction there.

The whole iron magnetic resistance may be written $\Sigma\frac{l\nu}{a\mu}$.

This is for only one of the magnetic circuits if the machine has more than one.

In a well-designed modern machine there is no throttling of the induction anywhere, and $\frac{l}{a_1\mu}$ may be taken as the whole magnetic iron resistance, μ being the permeability of the iron of the armature, l the average total length of a line of induction in the iron of the whole machine.

W watts, the power developed by the rotation of the armature.

W′, the permanent highest power developed.

$E\theta$, the rate of loss of heat in watts per square centim. of the cylindric outer surface of the armature. This is usually taken to be 0·2 in modern machines furnished with some ventilating arrangement. The value of $\sqrt{\frac{E\theta}{\rho}}$ may, we find, be practically taken as 288 in such machines; we shall call this constant q, as machines made by different makers differ greatly in its value, even where there are the same methods of ventilating and the same sizes of wires. With finer wires ρ is greater, as the space is less occupied by copper, so that q is less. If two thirds of the space be taken as occupied by copper, $q=288$ if $E\theta=0{\cdot}2$. In some armatures, which are always perhaps too safe from heating, we have found q to be as little as 150. In some cases it is considerably more than 300.

S_2, spires on the coils of the field-magnet part of one magnetic circuit. In the Edison-Hopkinson and Kapp machines there is only one magnetic circuit, and S' is here the total number of spires on the field-magnets; but in the Manchester, Crompton, A-Gramme, and other forms there are two magnetic circuits, and S' is here the number of spires on only one of the magnetic circuits.

A_2, amperes in each spire of the field magnet-coils; or

α', amperes per square centimetre of cross section of the winding.

Any person who has engaged in making measuring-instruments or dynamo machines is aware that for a given volume of winding, whether the wires are small or large, for the same distribution of temperature there will be the same number of ampere-turns and there will be the same rate of loss of energy by heating, if the volume of insulating material is always in the same ratio to the volume of the copper.

With fine wire the volume of insulating material becomes greater, but not to such an extent as to make useless this very important roughly correct practical rule for the makers of instruments and dynamo machines. This rule we have regularly used since 1881 in our measuring-instruments.

This has led us to speak of α the current in amperes per square centimetre of cross section of a coil, rather than the density of current in the copper alone, and we have been led to some general rules of considerable practical interest in consequence.

To make our rules more complete, we begin with one which is well known. It will be observed that we use Mr. Kapp's method of counting wires on the armature, so as to make the rule suitable both for the Hefner-Alteneck and the Gramme armature.

$$\text{Total E.M.F. of armature} = \frac{n\mathrm{SN}}{10^8} \quad . \quad . \quad . \quad . \quad . \quad . \qquad (1)$$

$$\left.\begin{array}{l}\text{E.M.F. developed in unit length}\\ \text{of wire passing through the field}\end{array}\right\} = \frac{vk\beta_1}{\pi 10} \quad . \quad . \qquad (2)$$

Observe that it is only the wire on the convex outer surface of the armature which is here considered.

$$\mathrm{W} = \frac{2n\mathrm{N}}{10^8}\mathrm{SA}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (3)$$

$$\mathrm{W} = \frac{2v\mathrm{N}t\alpha}{10^8}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (4)$$

The heat generated in the armature-winding per cubic centim. $=\rho\alpha^2$. The possible ventilation arrangements are better in Gramme than in Hefner-Alteneck armatures, and it is usual, therefore, to consider only the heat generated in the wires on the outside convex surface of the iron of either. This is $2\pi r t \mathrm{L}\alpha_1^2\rho$; and as the heat emitted is found, for the highest temperature θ at which it is considered safe permanently to work the machine, to be proportional to the convex surface $2\pi r\mathrm{L}$, if E is the rate of loss of heat in watts per square centimetre of this surface,

$$2\pi r t\mathrm{L}\rho\alpha_1^2 = \mathrm{E}\theta 2\pi r\mathrm{L},$$

or

$$t\rho\alpha_1^2 = \mathrm{E}\theta.$$

We find that $\dfrac{\mathrm{E}\theta}{\rho}$ may be taken as 83000 in the best modern machines; we shall call this q^2, so that

$$t\alpha_1^2 = q^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5).$$

We can, therefore, from (4), express the greatest permanent output of a machine W′, either in terms of α_1 or of t:

$$W' = 0\cdot00166\,\frac{vN'}{\alpha_1}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (6)$$

$$W' = \frac{2q}{10^8}\,vN'\sqrt{t}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (7)$$

$$W' = \frac{2q}{10^8}\,vkrL\beta_1'\sqrt{t}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

N' and β_1' being the highest allowable values of N and β_1.

It is on the combination of (5) and (4) that we have based an important generalization regarding the magnetic circuit of the dynamo, which gives the title to our paper. Members of the Society are aware of the methods adopted by Mr. Bosanquet, Dr. Hopkinson, and Mr. Kapp in dealing with a magnetic circuit. Consider a closed tube of small cross section everywhere, passing through the iron of the armature, the air-spaces, the limbs of the field-magnet, and the yoke. Let the section vary so that the total induction is everywhere the same. Then, in a short length of the tube l, the line-integral of the magnetic force required to produce this induction is

$$\frac{l}{\mu}\beta,$$

where μ, the magnetic permeability of the material at any place, is 1 for air, and where its value for iron is given in the accompanying figure for various values of β. The curve B, Pl. VIII. fig. 1, is that given by Mr. Bidwell in his paper (Proc. Royal Society, No. 245, 1886). The curve H_1 has been obtained from Dr. Hopkinson's experiments on wrought iron in the Philosophical Transactions, 1885, plate 47. We wish that Dr. Hopkinson had in this paper given his experimental numbers, instead of plotting them in curves to such scales that one feels very great uneasiness in making measurements. We have determined the curve H_1 by taking a medium curve between Dr. Hopkinson's curves for rising and falling magnetizations. It is quite probable that, in view of the great differences in the behaviour of the same and different kinds of iron, under different circumstances, Dr. Hopkinson did not feel that he was justified in drawing his curves on paper too finely divided. It will be observed that from $\beta=10000$ to $\beta=16300$ we may take $\mu=5221-0{\cdot}3071\beta$. Unfortunately for easy methods of calculation in many machines much larger values of β than 16300 are often used.

Curve H_2 is computed from Dr. Hopkinson's results for cast iron, and it shows how very important it is, when cast iron is used in the magnetic circuit of a machine, to have its section much greater than that of the wrought iron portions of the circuit, if throttling the induction is to be avoided.

Curves K_1, K_2, and K_3 are computed from the formulæ given in Mr. Kapp's paper for the wrought iron used in the armature, field-magnets, and yoke respectively of a dynamo machine.

The differences among these curves throw great light upon the fact that it is really impossible to predetermine the "*characteristic*" of a dynamo machine. Mr. Kapp gets over discrepancies by calculating a value of his leakage-resistance which suits the actual observations made on an already constructed dynamo machine; and Dr. Hopkinson's curves, although calculated on the basis of actual measurements of the leakage made on an already constructed dynamo, and although only computed for a very small portion of that part of the characteristic where the iron magnetic resistance is important (the only portion about which there is any difficulty), represent this small portion very indifferently indeed. Methods of calculating the leakage which have been put forward we can in no way believe in, for reasons given by one of us in the discussion on Mr. Kapp's paper at the Society of Telegraph Engineers. We are not in any way detracting from the merits of Dr. Hopkinson's beautiful theory. We are too well aware of the great services he has done us and of the enormous change which he has produced in the construction of dynamos.

Now the line-integral of the magnetic force round a closed magnetic circuit—called by Mr. Bosanquet the Magnetomotive Force—is the number of ampere-turns on the coils through which it threads it way $\times \frac{4\pi}{10}$. Hence if S_2A_2 be the ampere-turns on the field magnet,

$$\frac{4\pi}{10}S_2A_2=2\delta\beta+\Sigma\frac{l}{\mu}\beta, \quad . \quad . \quad . \quad . \quad . \quad . \qquad (9)$$

δ being $d+t$ the distance from iron of armature to iron of field-magnet, and of course the length of the tube in air is twice this distance as it goes into and out of the armature.

If N is the total induction through the iron of the armature, $\frac{N}{a_1}=\beta$ there and $\frac{N}{a_2}=\beta$ in the air-space.

If νN is the total induction through the iron of the field-magnet anywhere, then $\frac{\nu N}{a}=\beta$ there, so that (9) may be written

$$\frac{4\pi}{10}S_2A_2=2(d+t)\frac{N}{a_2}+\Sigma\frac{l\nu N}{a\mu},$$

or

$$N=\frac{\frac{4\pi}{10}S_2A_2}{\frac{2(d+t)}{a_2}+\Sigma\frac{l\nu}{a\mu}}; \quad . \quad . \quad . \quad . \quad . \quad . \qquad (10)$$

and as N is the induction through the armature produced by a magneto-motive force $\frac{4\pi}{10}S_2A_2$, equation 10 leads to $\frac{2(d+t)}{a^2}$ being called, by analogy with other physical resistances, the air magnetic resistance, and $\Sigma\frac{l\nu}{a\mu}$ the iron magnetic resistance of the circuit. The members of the Society will perhaps allow us to put in this way Dr. Hopkinson's theory, although he himself may object to some of the terms we use.

Now let it be assumed that there is a value of β for the armature iron which it is best to use in all machines when giving their permanent output. ν may also be taken as practically constant. We are aware that to both these assumptions exception may be taken, but this will not be found to affect the practical general result which we arrive at. Then μ is known, so that if the lengths and cross sections of the iron everywhere are known, the iron magnetic resistance $\Sigma\frac{l\nu}{a\mu}$ is known. Inserting this value of N as N′ in (7) we find

$$W' = \frac{2q}{10^8} v \sqrt{t} \frac{\frac{4\pi}{10} S_2 A_2}{\frac{2(d+t)}{a_2} + \Sigma \frac{lv}{a\mu}}; \quad . \quad . \quad . \quad (11)$$

and it will be found that this is a maximum for different values of t when

$$\frac{2t}{a_2} = \frac{2d}{a_2} + \Sigma \frac{lv}{a\mu}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

That is, when the magnetic resistance of the space occupied by the winding of the armature is equal to the resistance of the rest of the magnetic circuit.

When this is the case (11) reduces to

$$W' = \frac{2\pi q}{10^8} \frac{v a_2}{\sqrt{t}} S_2 A_2. \quad . \quad . \quad . \quad . \quad (13)$$

That the ampere-turns on the field-magnet S_2A_2 may really produce the given induction β_1 in the armature, it is necessary that

$$S_2 A_2 = \frac{10}{\pi} \frac{a_1 t}{a_2} \beta_1;$$

and if we insert this value in (13) we have of course (7) again.

It is now the custom in making dynamos to let β be nearly constant in all parts of the circuit where the iron is the same, and to have $\frac{\beta}{\mu}$ as nearly constant as possible, if different kinds of iron are employed. If $\frac{\beta}{\mu}$ is larger anywhere than its average value for the rest of the circuit, we say that the induction is there "throttled," and throttling is only allowable in the armature, if it is allowable anywhere. If $\frac{\beta}{\mu}$ is nearly constant everywhere in the iron, we may take $\frac{l}{a_1\mu}$ as the total iron resistance of the circuit, l being the average length of the lines of magnetic induction in the iron of the whole circuit, and then we may use

$$\frac{l}{a_1\mu} \text{ instead of } \Sigma \frac{lv}{a\mu}$$

in (10), (11), and (12); μ being the permeability of the armature iron when the induction is β_1'.

In the above investigation, the iron resistance is supposed to be given, and also the exciting power in ampere-turns. At present this seems a sufficiently practical basis for the calculation, as we usually fix first the size of armature, then arrange as short a magnetic circuit as possible, which must leave sufficient room for the exciting coils. We see, however, that a larger generalization is possible when we know with certainty what is the limiting thickness of winding on the field-magnets. We were allowed to assume in the armature winding that $t\alpha_1^2$ is constant, α_1 being the greatest permanent current per square centimetre allowable. When the winding is thin, so that the temperature is nearly uniform in the winding, and only when this is the case, is such a rule allowable, and it is not allowable in cases where the winding is of the thickness usual in field-magnet coils. We are at present experimenting on this subject, but there are considerable difficulties in the way of obtaining practical rules. We have no doubt, however, that there is such a rule as

$$S_2'A_2'=p\lambda.$$

That is, the greatest number of ampere-turns which can usefully and permanently be applied on a field-magnet coil of length λ is proportional to λ. If l is given, the configuration of the machine enables λ and therefore $S_2'A_2'$ to be fixed. Then $S_2'A_2' \div \frac{2l}{\mu_1} = \beta_1$ enables β_1 and μ_1 to be calculated by trial if there is sufficient information about the character of the iron. $\frac{2t}{a_2}$ is now made equal to $\frac{2d}{a_2} + \frac{l}{a_1\mu_1}$, so that the important dimensions are fixed. Should β_1 be great, considerations of cost of the field-magnet winding may come in, to alter completely the design of the machine, but for a given configuration of machine this is the practical method of working.

From experiments, the results of which were published before this Society on March 12th, 1887, we came to the conclusion that there is a definite resistance at a joint in a magnetic circuit; and Professor Ewing found that by cutting a bar into two, four, and eight pieces, the magnetic permeability seemed to alter from 1220 to 980, 640, and 480 respectively, the joints being tooled up in the usual way. When the joints are carefully scraped he also found that they materially increased the magnetic resistance unless considerable pressure was applied. Now as a stress of less than one quarter of the ordinary stresses to which wrought iron is subjected in machinery materially diminishes the permeability of iron, it is obvious that all joints in the magnetic circuit ought, as much as possible, to be got rid of, consistently with easy manufacture; or, when they exist, they ought to be made of as large area as possible, by letting one piece into another to a considerable depth.

In the design of a machine, the question of possible leakage is of considerable importance. It will often be found that in the effort to make l short and a_2 large, machines are designed with enormous amounts of leakage not only from one pole to the other, but from either pole to the middle of a neighbouring field-magnet coil.

One of the best ways of finding the nature of the probable magnetic leakage in a dynamo, before it is constructed, is to construct a small model of the same kind of iron, exciting the field-magnets, and exploring by means of a ballistic galvanometer. Another simpler way, which gives a considerable amount of information, and which we have employed, is to make a model of wood, covering certain judiciously selected parts, such as the poles and armature, and half the field-magnets with metal, immerse the model in a barrel of rain-water, and find the electric resistance between one part and another when electric potential-differences are established between them. On account of the ease with which the model may be rearranged in configuration, this method of working gives interesting results; but these results, when applied in the magnetic case, must be used judiciously and with the knowledge that the permeability of iron is not a fixed quantity.

It is obvious that the best section of a field-magnet limb is the circular, but considerations of possible leakage to the middle of the limb from the armature or a pole-piece, and other considerations relating to the configuration of the machine, often cause us to give to the section a rectangular or oval section.

The Characteristic of a Dynamo.

In a letter written by one of us from Japan, in January

1879*, before he had seen a dynamo, the necessity was shown for establishing an algebraic relation between the E.M.F. developed in the armature and the current exciting the field-magnets. It was pointed out that E the E.M.F. in the armature was proportional to the field and speed, and that this led to

$$\mathrm{E}=p+qc+\Sigma\frac{ac}{b+c} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

for any given speed. It was also pointed out that a tangent function might also be employed, but that it would not lend itself so readily to calculation. We had, both, in 1878 used a curve to express this relationship, but it was not until 1881, when we met M. Deprez, and learnt of his work, that we had any conception of the many calculations which might be made by graphical methods, using the curve as a fundamental relation. Herr Fröhlich uses one term of the above expression:—

$$\mathrm{E}=\frac{ac}{b+c} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

He regards p the permanent field as 0, and he has no term which remains proportional to the current. We shall speak of (15) as the Fröhlich formula, because it usually goes under that name. It is, of course, an empirical formula, like (14).

The reasoning which led us to regard (14) as a *rational* formula, was based on a magnetic theory which need not here be expounded.

A very great necessity exists for having some such empirical formula as (15), but, as is well known, it is quite incorrect when c is small. It is not nearly so incorrect as we might imagine it to be from the statements made by Mr. Kapp (p. 529, Journ. Soc. Tel. Eng. and Elect. vol. xv. 1887), who has given in his fig. 3 a most absurdly unsuitable Fröhlich curve, or by Dr. Hopkinson (plate xvi. Phil. Trans. 1886), whose Fröhlich is also not the most suitable. It is obvious that the Fröhlich ought not to be expected to agree with the real characteristic near the origin. It ought to be made to agree most perfectly with the working part of the characteristic. To effect this purpose let the observed values of $\frac{E}{C}$ and E be plotted as the coordinates of points on squared paper. The straight line which lies most evenly among the points for the working values of E satisfies the equation

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* Not published till 1885, and then in a somewhat mutilated form in the 'Electrician' of November 20.

$$\frac{E}{C}b+E=a.$$

The measured coordinates of two points on this straight line enable b and a to be calculated.

Thus, for example,

$$N=\frac{1364\,F}{1+2\cdot7\times10^{-4}\,F}$$

will be found to satisfy the observations published by the Drs. Hopkinson for the Manchester dynamo from $F=6000$ to $F=30{,}000$ with a wonderful amount of accuracy. But for the early part of the curve it is quite unsuitable. F is $\frac{4\pi}{10}\times$ ampere-turns in one coil of the machine, or the magneto-motive force.

The uses made by Herr Fröhlich and Prof. Rücker of (15) in a theory of compounding show how important it is to have a simple empirical formula. Indeed in our patent specification of 1882 we base the theory of compounding on the simplest of all empirical formulæ,

$$E=aC. \qquad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (16)$$

And it is by means of this formula that the theory can be best put before students; if our easy reasoning is properly grasped, the practical electrical engineer will be able to use the results graphically with actual characteristics in the manner employed by the most experienced men at the present day.

Instead of speaking of E the E.M.F. produced in the armature, which is proportional to N the magnetic field and n the speed, as shown in (1) it is preferable to speak of N the field itself. And instead of referring merely to the current exciting the field-magnets, it is preferable to speak of the ampere-turns S_2A_2, or, better, of the magneto-motive force in C.G.S. units, $\frac{4\pi}{10}S_2A_2$.

We have tried in vain, during perhaps two months of very hard work, to express the two constants a and b of (15) in terms of the dimensions of a dynamo machine. We now believe that this cannot be effected. We have already given the reason why the linear law connecting the β and μ cannot be used in obtaining a formula. In any case such a formula would be very different from that of Fröhlich. But in view of the important relation (12) which we have established, that when the machine is giving its best permanent output,—

> The air magnetic resistance of the space occupied by winding on the armature = all other magnetic resistance,

it is not difficult to arrive at practical rules of working. It will be noticed that in existing machines we may, in general calculations, neglect the resistance of the clearance-space *d*.

The most important fact to be kept in mind by constructors of dynamos is, that the magnetic air-resistance is the governing factor. Until half the highest induction is reached we may neglect altogether the resistance of the iron; and indeed for many calculations it is sufficient to take

$$N = \frac{4\pi}{10} \frac{a_2}{2\delta} S_2A_2 \; ; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

where a_2 is the area of the pole-piece exposed to the armature (increased by the fringe-area, which Dr. Hopkinson estimates to be of a breadth $0{\cdot}8\,\delta$), and δ is the distance from iron of armature to iron of pole-piece.

(17) gives, in fact, the straight part of the characteristic. In well-constructed dynamos the magnetic iron resistance is equal to the air resistance $\frac{2\delta}{a_2}$ when the machine is working at its most permanent output; and it is only necessary to know β', the induction convenient to use when this is the case, to be able to calculate N and S_2A_2 for a point on the curved part of the characteristic. Thus

$$a_1\beta_1 = N_1 = \frac{4\pi}{10} \frac{a_2}{4\delta} S_2A_2;$$

so that if a_1 and β_1 are known, S_2A_2 can be found. Thus in the Manchester dynamo described by Dr. Hopkinson (Trans. Roy. Soc. 1886),

$a_1 = 220{\cdot}5$ square centim.,
$a_2 = 839{\cdot}5$ " "
$\delta = 0{\cdot}8$ centim.

Taking $\beta_1=18{,}460$ lines per square centim., we find

$$a_1\beta_1=4{\cdot}07\times10^6.$$

Now

$$\frac{4\pi}{10}\frac{a_2}{2\delta}=660.$$

So that $N=660\ S_2A_2$ represents the straight part of the characteristic.

Again, $$N_1=a_1\beta_1=4{\cdot}07\times10^6,$$
and
$$a_1\beta_1=330\ S_2A_2,$$
or
$$S_2A_2=\frac{4{\cdot}07\times10^6}{330}=12330.$$

Plotting $N=4{\cdot}07\times10^6$ and $S_2A_2=12330$ as the coordinates of a point on squared paper, and plotting also the straight line from the origin $N=660\ S_2A_2$, a man who has seen characteristics before will be able to draw the characteristic of this machine with a fair amount of accuracy, especially if he recollects that the iron is near saturation. Those, however, who have less experience may find the Fröhlich, which for any dynamo passes through the point on the straight line which represents $N=10{,}000\ a_1$, and the second point just found, rounding off the straight line into the Fröhlich by hand. In applying our rule to the Manchester dynamo we employed neither of these methods, as one's judgment is vitiated when one has seen the actual observations; and we have assumed that the very roughly correct rule,

$$N=p\tan^{-1}(h\,.\,S_2A_2),$$

may be applied, making its slope the same as that of the straight line at the origin, and making it pass through the point whose position we have calculated. In this case

$$ph=660\,;$$

and it will be found that

$$p=34{\cdot}65\times10^5,$$

and

$$h=19{\cdot}03\times10^{-5}$$

for this case.

As Dr. Hopkinson has given his characteristics, not in terms of ampere-turns S_2A_2, but in absolute units, calling

$\frac{4\pi}{10}S_2A_2$ by the letter F to denote the magneto-motive force in absolute units, the above formula becomes for this comparison,

$$N=34{\cdot}65\times10^6\tan^{-1}(15{\cdot}15\times10^{-5}F).$$

In the figure we have plotted the curve obtained by calculation as A; B shows Dr. Hopkinson's calculated curves; the dotted line passes as nearly as possible through the points determined by experiment. It will be observed that the Drs. Hopkinson have only ventured to calculate the characteristic up to F=12,000 on the ascending, and 9500 on the descending part; whereas the observations extend to F=29,500. As the very straight part near the origin presents no difficulty, it may be said that all their elaborate calculation represents only the characteristic from about F=6000 to F=12,000, and that part very indifferently. It is, besides, to be observed that in an actual dynamo there are no such differences between observations made with steadily increasing and with steadily diminishing magneto-motive forces, as the theory requires.

In the above calculation we have neglected the clearance *d*, as we did not know it. It would have been easy to calculate a value of *d*, possibly not very different from the real value, to make our curve agree more closely with the observations. In conclusion we would say that, in our opinion, it is impossible, until a machine is constructed, to compute its characteristic with sufficient accuracy for such purposes as the determination of coils in compound winding. But the probable characteristic may be determined in the way here described, with sufficient accuracy for a number of useful purposes. It is based on the facts:—(1) When a machine is working at its best permanent output, its iron magnetic resistance plus the air magnetic resistance of the clearance is equal to the air magnetic resistance of the space on the outside of the armature occupied by winding. (2) At the beginning the air-resistance is alone of any importance.

XXXI. *The Variation of the Coefficients of Induction.* By W. E. Sumpner, *B.Sc., Assistant in the Physical Department, Central Institute, South Kensington*[*].

[Plate IX.]

1. There are three ways of defining the coefficient of self-induction, which lead to the same result if the magnetic permeability of the medium is a constant quantity, but which lead to three different results if it is a variable one, as in the case of iron.

The coefficient of self-induction L of a coil of wire through which a current C is passing may be defined as the ratio between the back electromotive force and the time-rate of change of the current C to which it is due; or it may be defined as the ratio between the flux of induction through the coil and the current producing it; or it may be defined with reference to the electrokinetic energy possessed by the current C.

The three definitions are expressed by the equations

$$(1) \qquad e = L_1 \frac{dC}{dt},$$

$$(2) \qquad N = L_2 C \quad \text{and} \quad e = \frac{d(L_2 C)}{dt},$$

$$(3) \qquad T = \tfrac{1}{2} L_3 C^2.$$

In these equations e is the back electromotive force produced by varying the current C, N is the number of lines of force passing through the coil, T is its kinetic energy, and L its coefficient of self-induction.

2. If the magnetic medium is air, L_1, L_2, and L_3 are identical. If the medium be wholly or partially composed of iron, this is no longer the case. The values of L differ from each other, and vary with C. The value of L_1 can easily be found in terms of L_2 from the equation

$$L_1 = L_2 + C \frac{dL_2}{dC},$$

which is an immediate consequence of the first two equations of definition.

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[*] Read April 14, 1888.

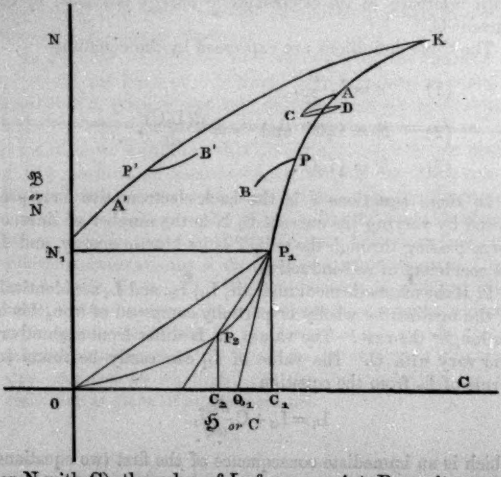
We see that $L_1 = L_2$ only when $C = 0$ or when $\frac{dL_2}{dC} = 0$, and that L_1 is greater than L_2 for small currents where L_2 is increasing with C.

Since $\mathfrak{H}$, the magnetizing force, is proportional to C; and since N is a measure of $\mathfrak{B}$, the average value of the flux of induction per unit area, we see that L_2 is directly proportional to μ, the average value of the coefficient of magnetic permeability for the magnetizing force represented by the current C.

If, therefore, the medium be wholly of one kind, L_2 will be a measure of the permeability of that medium. In any case, however, if we know the relation connecting $\mathfrak{B}$ with $\mathfrak{H}$, we shall be able to determine the way in which the coefficients vary with the magnetizing force.

If OPKP′ (see fig. 1) be the curve connecting $\mathfrak{B}$ with $\mathfrak{H}$

Fig. 1.



(or N with C), the value of L_2 for any point P_1 on it corresponding with the current OC_1 and the flux of induction ON_1

Fig. 2.

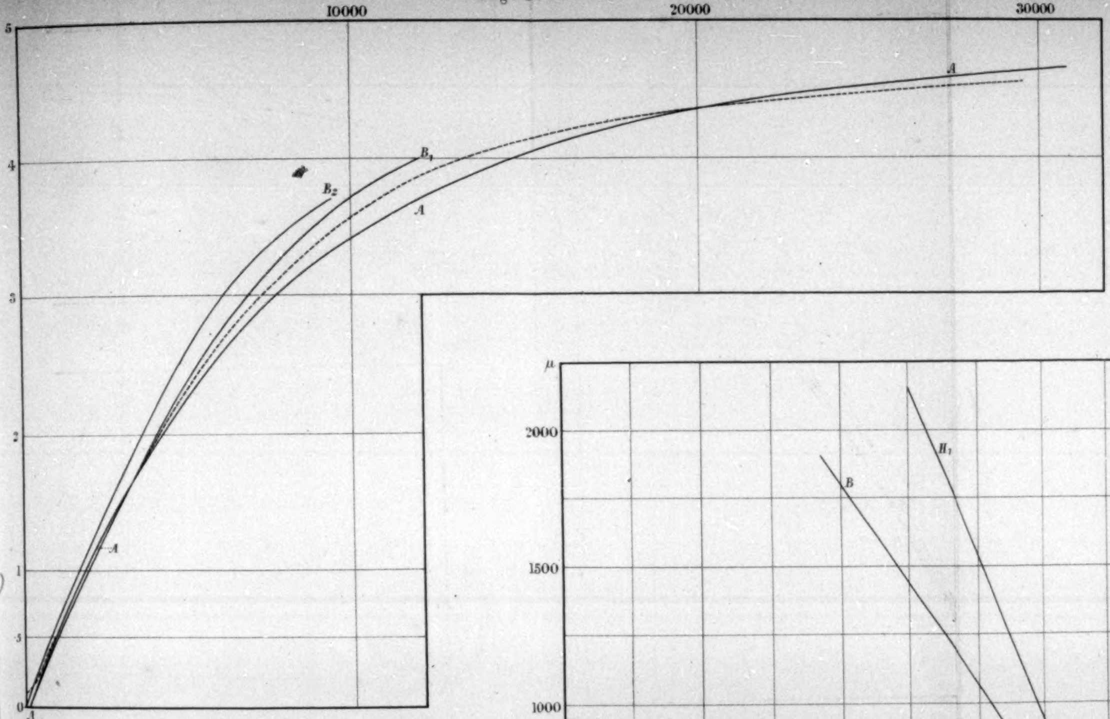
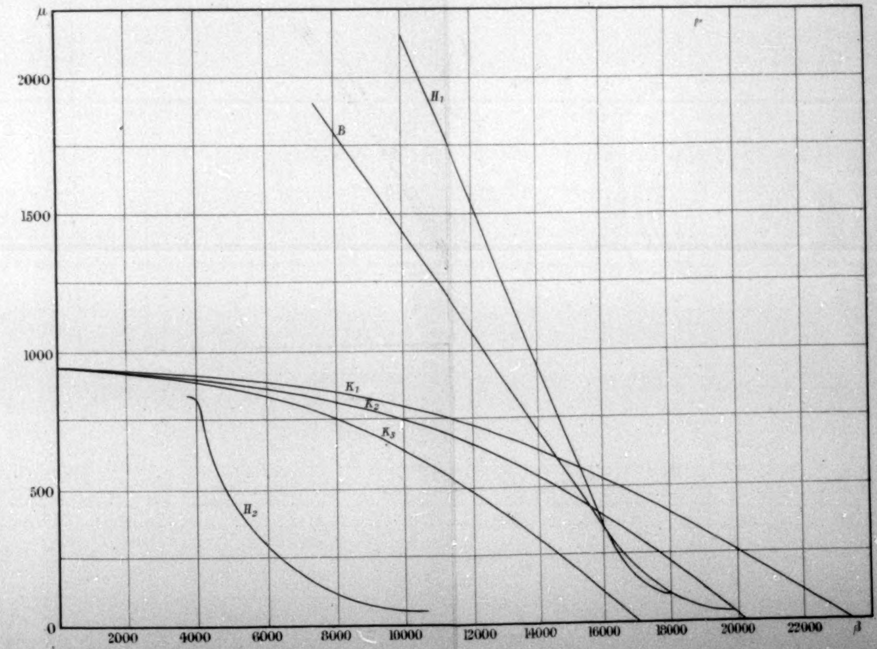


Fig. 1.



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will be represented by the tangent of the angle which the line OP_1 makes with the line OC_1.

Since

$$L_1\frac{dC}{dt}=e=\frac{d(L_2C)}{dt}=\frac{dN}{dt},$$

we have

$$L_1=\frac{dN}{dC}\propto\frac{d\mathfrak{B}}{d\mathfrak{H}},$$

and L_1 will consequently be represented by the tangent of the angle $P_1Q_1C_1$ which the tangent to the curve at P_1 makes with the line OC_1.

Moreover,

$$L_3=\frac{2\int CdN}{C^2}\propto\frac{\int\mathfrak{H}d\mathfrak{B}}{\mathfrak{H}^2},$$

and will be represented by the ratio of the area enclosed by the lines ON_1, P_1N_1, and the curve OP_1 to the square on the line OC_1.

3. Maxwell's method of determining the coefficient gives L_2, because the quantity of electricity discharged through the galvanometer is proportional to the number of lines of force inserted by the establishment of the current C (or to the number removed by stopping the current), while the steady deflection due to a small derangement is proportional to C directly. The method therefore gives the ratio of N/C or L_2. Professor Ayrton has generalized this method by altering the current from one value to another, instead of establishing it from zero to its full value. The quantity discharged through the galvanometer is proportional in this case to N_1-N_2, where N_1 is the number of lines of force corresponding with the first current C_1, and N_2 that corresponding with the other current C_2. If the steady deflection caused by a derangement from balance be taken when a current C is flowing through the coil, Maxwell's formula will give the ratio of $\frac{N_1-N_2}{C}$; and if we multiply this by $\frac{C}{C_1-C_2}$, we shall obtain the value of $\frac{N_1-N_2}{C_1-C_2}$, or the tangent of the angle which the chord P_1P_2 makes with the line OC.

By observing the ratio $\frac{C_1-C_2}{C}$, in addition to the ordinary quantities measured in Maxwell's method, it is possible to determine in this way the rate of slope of the curve of magnetization. If (C_1-C_2) is very small compared with $\frac{1}{2}(C_1+C_2)$, the value obtained by Prof. Ayrton's method is L_1; but the result is practically the same if the curve of magnetization does not bend very much between the values of OC corresponding with C_1 and C_2.

Coefficients for small Currents.

4. It follows from the shape of the initial part of the curve of magnetization that the value of L_2 must at first increase with the current. The following experiments show that this is the case. A bar of best Swedish iron, 14 in. long by ½ in. in diameter, was bent into the shape of a horseshoe. Each end was surrounded by a bobbin, 2½ in. in diameter and 4½ in. long, wound with 400 turns of wire. The self-induction of the two bobbins in series was obtained by Maxwell's method. The battery used was an ordinary Leclanché cell. The swings were in most cases taken on breaking the circuit, since, if the galvanometer-key be pressed just before the battery-key is released, the error caused by a slight want of balance in the bridge is reduced to a minimum. For a similar reason, the deflection corresponding with a known derangement was always estimated from the difference between two deflections. The current flowing through the coil was calculated from the electromotive force of the battery and the resistances in the network. The following numbers represent the mean of several concordant values :—

A.	L_2.	$L_2-\cdot 2A$.
·047	·0514	·0420
·056	·0539	·0428
·060	·0549	·0429
·065	·0554	·0424
·068	·0564	·0428
·079	·0577	·0423
·091	·0609	·0427
·107	·0634	·0420

In this table L_2 is the coefficient of self-induction of the coil in secohms, and A is the current in amperes flowing through it. The experiments are in fair accordance with the relation

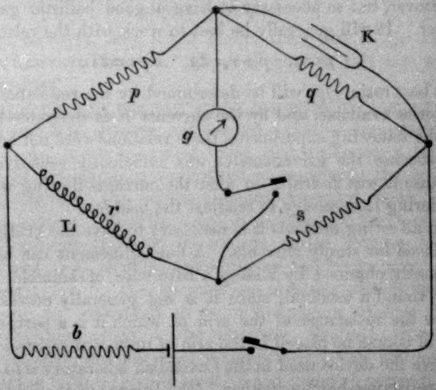
$$L_2 = \cdot 2A + \cdot 0425.$$

Since A is proportional to $\mathfrak{H}$, and L_2 is proportional to $\mathfrak{B}/\mathfrak{H}$, the above experiments tend to show that the part of the curve of magnetization between the points corresponding with the currents ·047 and ·107 ampere is a parabola of the form

$$\mathfrak{B} = a\mathfrak{H}^2 + b\mathfrak{H}.$$

5. It was, however, desirable to see whether this relation would still hold good for smaller magnetizing forces. With this view the experiments were repeated some months afterwards in a somewhat different way. The coefficient was determined by comparing it with the capacity of a standard condenser. Two arms, *p*, *q*, of a Wheatstone bridge (see fig. 2) consisted of doubly wound resistance-coils of 10,000 ohms each. The resistance *g* of the galvanometer used was also about 10,000 ohms. To the coil *q* was shunted a condenser of $\frac{1}{3}$ microfarad capacity. The opposite branch *r* of the bridge contained the electromagnet whose self-induction was required. The resistance of the arm *s* was 5 ohms, and that of *r* was adjusted till there was balance. The battery used was

Fig. 2.



an accumulator. The currents were changed by inserting more or less resistance into the battery-circuit. If the bridge is balanced for steady currents, and the battery-circuit be opened, the swing θ_1 produced will be proportional to $L_2 - Kps$, where L_2 is the coefficient required and K is the known capacity. If one of the terminals of the condenser be now disconnected from the bridge, and a second swing θ_2 be taken on breaking circuit, we have

$$\frac{\theta_2}{\theta_1} = \frac{L_2}{L_2 - Kps},$$

or

$$L_2 = \frac{\theta_2}{\theta_2 - \theta_1} Kps.$$

This method, although only comparative, has several advantages over Maxwell's absolute method. It is quicker and simpler ; there are only two quantities to observe, and the readings may be taken immediately after each other. It is, moreover, not so necessary to have a good ballistic galvanometer. It will generally be best to work with the relations

$$p = q, \quad p + r = 2g, \quad Kps = 2L.$$

The best ratio, r/p, will be determined by the resistances and batteries available, and by the currents it is desirable to use. In the following experiments these relations were not adhered to, because the galvanometer was sufficiently sensitive, and because it was desirable to alter the currents flowing without rendering it necessary to readjust the bridge.

In all swing methods it is necessary to have the bridge well balanced for steady currents. A fine adjustment can be conveniently obtained by sliding a bare wire of suitable thickness round a terminal, since it is not generally necessary to know the resistance of the arm of which it is a part, and it can of course be placed in the arm of unknown resistance. I believe the device used in the Cavendish laboratory is to shunt one resistance-box by another. The balance of the bridge has to be so good that the heating caused by the momentary passage of the current is often sufficient to destroy it.

The annexed table indicates the results obtained with the electromagnet with the iron core. The swings were obtained by breaking the battery-circuit.

b.	θ_2.	θ_1.	L_2.	A.
0	688	508	·0637	·220
1	627	462	·0634	·200
5	444	321	·0606	·147
10	367	261	·0577	·110
20	230	160	·0547	·073
30	164	111	·0517	·055
100	49·5	29·75	·0419	·020
130	38·0	22·7	·0414	·016
200	24	13·5	·0381	·0105
230	20·5	11·0	·0360	·0092

The values obtained for L_2 are slightly lower than the corresponding values obtained by Maxwell's method three months previously. This is probably due to the fact that the electromotive forces of the cells used were only approximately determined. The differences, however, are small, and may have been due to alterations in the state of the iron. The values for L_2 obtained for small currents are seen to be much lower than those given by the equation

$$L_2 = \cdot 2A + \cdot 0425.$$

They are, however, very much higher than that of the coil with the iron core removed. The coefficient in this case was ·0028 secohms.

6. The coefficient was very much increased by keeping a piece of soft iron pressed against the poles of the electromagnet. As, however, under these circumstances, a large fraction of the magnetism remained on removing the magnetizing current, the values obtained depended upon the previous history. Thus the value of L_2 obtained for a magnetizing current of about ·04 ampere varied between ·337 secohm and ·130 secohm, according as the flings were obtained on reversing the current, on breaking the battery-circuit, or on making it so as to include or exclude the permanent magnetism. The numbers obtained showed, moreover, that the curve of magnetization was not quite symmetrical with respect to positive and negative currents.

The following table gives the values of L_2 obtained for different currents by the method of reversals. The resistances and electromotive forces used to obtain the last five observations were quite different from those used to obtain the first six.

A.	L_2.	$\mathfrak{B}$.	μ.	$\mathfrak{H}$.
·0366	·184	665	780	0·852
·0275	·156	424	661	·640
·0220	·141	306	597	·512
·0147	·112	163	474	·342
·0110	·091	99	385	·256
·0055	·071	38·6	301	·128
·0029	·062	17·8	263	·068
·0044	·066	28·7	279	·102
·0100	·091	89·8	385	·233
·0200	·128	253	542	·465
·0400	·189	746	801	·931

7. As the magnetic circuit in this case was entirely composed of iron, it was easy to reduce the observations to absolute measure. The values of the induction $\mathfrak{B}$, the magnetic force $\mathfrak{H}$, and the magnetic permeability μ were calculated in C.G.S. units from the formulæ

$$\mathfrak{B} = \frac{10^8}{nS} L_2 A, \quad \mathfrak{H} = \frac{4\pi n}{10 l} A, \quad \mu = \frac{10^9 l}{4\pi n^2 S} L_2,$$

where A was the current in amperes, L_2 the coefficient of self-induction in secohms, S and l the mean values of the sectional area and length of the magnetic circuit in centimetres, and n the number of turns.

The iron core was half an inch in diameter and 14 inches long, and the distance between the poles was 3 inches. The number of turns was 800. Whence

$$l = 17 \times 2{\cdot}54 = 43{\cdot}2, \quad S = \frac{\pi}{4}\left(\frac{2{\cdot}54}{2}\right)^2 = 1{\cdot}27, \quad n = 800.$$

If these values are substituted, we obtain

$$\mathfrak{B} = 98{,}700\, L_2 A, \quad \mathfrak{H} = 23{\cdot}3\, A, \quad \mu = 4{,}240\, L_2.$$

The numbers obtained very approximately fulfil the following relations:—

$$L_2 = 0{\cdot}05 + 3{\cdot}9\, A,$$
$$\mu = 210 + 720\, \mathfrak{H},$$
$$\mathfrak{B} = 210\, \mathfrak{H} + 720\, \mathfrak{H}^2.$$

The values were obtained for reversals of magnetizing force whose semiamplitude $\mathfrak{H}$ was greater than 0·06 and less than 0·9 C.G.S. units. Beyond these limits the value of $\mathfrak{B}$ probably differs from that given by the above relation. By employing the secohmmeter of Professors Ayrton and Perry, it would have been possible to obtain values of L_2 for much smaller magnetizing forces than those used above, and therefore for fields very much weaker than that due to the earth's magnetism.

Coefficients for large Currents.

8. The coefficient of self-induction of a coil with an iron magnetic circuit can far more easily be obtained for strong magnetic forces when the magnetism is maintained by an independent magnetic field than when the magnetism of the iron has to be excited by the current through the coil itself. In a transformer, for instance, where there are two coils identically situated with respect to the iron circuit, the coefficients of self and mutual induction are in the ratio

$$n_1^2 : n_2^2 : n_1 n_2,$$

where the ratio of the turns on the first coil to that on the second is $n_1 : n_2$. This will be true whatever the state of magnetization. If, therefore, one coil is used to excite the magnetism, and the other to test it, the values of the coefficients can be obtained without the least difficulty. Again, it is easy to obtain by Maxwell's method the value of L for the armature of a Series dynamo when the field-magnet coils are used independently to excite magnetism in the armature. Experiments were made in February 1887, at Prof. Ayrton's suggestion, to determine the coefficient of self-induction of the armature of a Gramme dynamo of the A pattern. The numbers obtained by Mr. S. Watney and the writer were as follow :—

Amperes round field-magnets.	Self-induction, in secohms.
0·0	·0218
6·1	·0179
15·1	·0135
24	·0122
29	·0117

9. When, however, it is desired to determine the self-induction of the field-magnet coils of a dynamo when a strong current is flowing through them, several difficulties present themselves. If strong currents are to be maintained the resistances must be small, unless a large amount of power is to be wasted and unless very high electromotive forces are procurable. On the

other hand, the self-induction to be measured is very large. The time-constant, which determines the rate at which the magnetism will change, will therefore be large; and unless a sufficiently ballistic galvanometer is to be had, it is not easy to obtain L in absolute measure.

At the suggestion of Prof. Ayrton some experiments were made at the commencement of 1887 to determine the variation of the self-induction of the field-magnet coils of a Ferranti dynamo. The method* used was the modification of Maxwell's method already alluded to. (The coefficient has been since compared with the capacity of a standard condenser by the swing method; the value obtained for a current of 0·01 ampere was 0·61 secohm, and for larger currents larger values were obtained.) When a strong current was flowing through the coils the resistance of the circuit was only a few ohms, and the coefficient was probably larger than 0·6. The time-constant was therefore generally larger than one tenth of a second, and the discharge could never be considered completed in less than a second. Probably no galvanometer would have been sufficiently ballistic under these circumstances to give good results in absolute measure. As, however, the only galvanometer conveniently situated with reference to the dynamos was one of the D'Arsonval type, with a short period and large logarithmic decrement even when on open circuit, the hope of any but comparative measurements was abandoned.

10. It is possible to calibrate any galvanometer for ballistic purposes by charging a condenser to a standard potential and discharging it through a resistance in series with the galvanometer to be calibrated. By suitably altering the resistance or the capacity the time-constant of discharge may be made to have any value. This was done with the D'Arsonval galvanometer, in order to interpret the results obtained. A condenser of 18·6 microfarads capacity was charged by a Latimer-Clark's cell, and then discharged through the galvanometer (whose resistance was 700 ohms) in series with a variable resistance, R ohms. The time-constant of discharge was

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* This method, due to Prof. Ayrton, will be found more fully described, together with several other methods, in a paper by the present writer on "The Measurement of Self-Induction, Mutual Induction, and Capacity," Journ. Soc. Tel. Engineers, May 1887.

therefore

$$18{\cdot}6(\mathrm{R}+700)10^{-6} \text{ seconds},$$

and the ratio of this to 1·66 was the ratio of the time-constant to the period of the galvanometer, and is denoted in the following table by the letter ρ. Observations were made of the value of the first swing of the galvanometer-needle for many different values of R. A few of these are given below.

R.	ρ.	First Swing (mean).
0	·008	293·5
1000	·019	291·2
5000	·064	269·0
10000	·120	234·0
50000	·568	105·5
100000	1·128	61·5
200000	2·248	36·0
300000	3·368	25·0

Thus, for the particular galvanometer tested, the throw produced by a given discharge when the time-constant is one tenth of the period is only about 80 per cent. of that produced by the instantaneous discharge of the same quantity; and when the time-constant is equal to the period, the throw is only about 20 per cent. of the corresponding throw for instantaneous discharge. Experiments since made with a high-resistance Thomson astatic galvanometer, with a period of 10 seconds, have yielded practically the same results. In the actual experiments on the Ferranti field-magnet coils, it would have been possible to interpret the meaning of the galvanometer-swings if the time-constant of discharge had had any fixed value, or if this value had been known; but the resistances and self-induction were continually varying, and the self-induction was always unknown. It would probably have been impossible even to compare the results with each other had it not been for the fact that, as the currents increased, the resistance and self-induction of the circuit diminished simultaneously, so that the time-constant of discharge tended to remain fixed in value.

11. The experiments on the dynamo coils were carried out by Messrs. Rossiter and Watney together with the writer.

The electromotive force used was obtained from accumulators, and amounted to about 100 volts. The bridge was kept balanced for steady currents. The only resistance altered was that in the battery-circuit. The currents were changed by switching resistances into or out of the battery-circuit. If Q is the quantity of electricity discharged through the galvanometer when the current in the coil changes from C_1 to C_2,

$$L_1 = K \frac{Q}{C_1 - C_2},$$

where K is a function of resistances only, and is independent of the resistance of the battery branch when the bridge is balanced for steady currents. If the discharge Q produces a throw θ,

$$Q = k\theta,$$

where k depends, among other things, on the time-constant of discharge, and therefore varies with the self-induction and with the resistance in the battery branch. We may, however, regard k as being approximately constant, and we may accordingly take the values of $\frac{\theta}{C_1 - C_2}$ to represent the values of the coefficient for the current $\frac{1}{2}(C_1 + C_2)$. In a series of experiments the currents were changed by successive steps from a small value to about 13 amperes, and decreased through the same stages in inverse order. The cycle was repeated many times, both for positive and for negative currents; and ~~the~~ numbers given in the following table are a fair sample of the results obtained. L_i and L_d are the values of $\theta/(C_1 - C_2)$ for increasing and decreasing currents respectively. A is the value in amperes of $\frac{1}{2}(C_1 + C_2)$.

A.	L_i.	L_d.
0·21	98	137
0·69	115	143
1·13	118	133
1·65	126	131
2·27	126	128
2·75	131	132
3·04	132	137
3·39	113	111
3·91	113	111
4·60	104	100
5·60	106	101
7·15	78	72
9·70	61	51·5

The numbers obtained, although not altogether satisfactory, leave no doubt about the way in which the coefficients change. For increasing currents L_i at first increases and then diminishes, while for decreasing currents L_d begins at a lower value than the corresponding value of L_i and continually increases as the current is decreased. This is exactly what is to be expected when it is remembered that the value of L is represented by the slope of the curve of magnetization. If the curve connecting $\mathfrak{B}$ with $\mathfrak{H}$ be like that indicated in fig. 1, the curve connecting $d\mathfrak{B}/d\mathfrak{H}$ or L with $\mathfrak{H}$ will be such that for increasing values of $\mathfrak{H}$ the ordinates L will at first increase and then diminish, while for decreasing values of $\mathfrak{H}$ the ordinates will continually increase. It is noteworthy that a discontinuity will occur at the cusp K, and the value of L will suddenly diminish as the change of current alters from an increase to a decrease, or *vice versâ*.

The general shape of the curve connecting L with $\mathfrak{H}$ will be the same whether the value of $\mathfrak{H}$ has been diminished to zero from positive or from negative values before the experiments are made. This follows from the fact that the slope at a point on a Ewing's cycle increases from each cusp to the point of inflexion. The coefficient L will be a maximum for those values of the magnetizing current C at which the points of inflexion occur. As the cycle only has one point of inflexion on each branch, the two parts of the curve connecting L with C will each have one maximum point, and one only. The two maximum values of L will not, however, occur at the same value of the current. They will occur at a small positive value of C for currents changing in the positive direction, and at a small negative value of C for currents changing in the negative direction. From this it follows that for increasing currents, L will at first increase and then diminish; while for currents decreasing to zero, L will continually increase. This statement must be modified for small cycles, which do not generally have points of inflexion. Here the coefficient will increase continually from one cusp to the other, and decrease discontinuously on rounding each cusp.

Experiments were also made on the field-magnets of a Gramme dynamo, the currents used varying in this case up to 30 amperes. The same kind of results were obtained.

12. Several remarkable effects were obtained, due to previous history of magnetization, which are easily explicable on reference to the researches of Professor Ewing*.

Suppose the current has been altered until a point P (see fig. 1) on the curve of magnetization is reached. The effect of a small change of current will now depend upon whether the current be increased or diminished. If the current be changed in the same way as it was altered last, a point A on the curve of magnetization will be reached; while if it be changed in the opposite direction, a point B will be reached. The slope of the line PA will be quite different from that of PB, and as these slopes represent the values of the coefficient of self-induction at the point P, it follows that this coefficient has always two distinct values whatever the state of magnetization and however that state has been attained. The curves obtained by Professor Ewing imply that the coefficient is greater for a change of current in the same direction as the last than for one in the opposite direction; for if P be any point on the ascending curve of magnetization, and P′ any point on the descending curve, the slopes PA and P′A′ correspond with changes of current in the same direction as those immediately preceding, and these slopes are steeper than those of PB and P′B′, which correspond with reverse changes of the magnetizing force.

If the current be changed between the two values corresponding with the points P and A, and the swings of the ballistic galvanometer observed for each alternation, the first fling will correspond with the slope PA, if the magnetization has been increased up to the point P. All the succeeding swings will, however, correspond with more gradual slopes, AC, because the magnetization owing to hysteresis will not return to the state P, but will proceed around a Ewing's cycle between the two points A and C, if the current be alternated between the two values considered. In the case of a transformer, two or three successively diminishing swings were often observed before the changes became cyclic. Whether on the ascending or descending portions of the magnetization-curve, the first fling should be greater than the succeeding ones if it corresponds with a change of current of the same kind as that immediately preceding.

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* "Experimental Researches in Magnetism," Phil. Trans. part ii. 1885.

The following experiments on the Ferranti field-magnets illustrate these remarks:—

Previous history.	Current Change.	Successive Swings.
C increased to +12 amperes, and diminished to 5·86.	5·86 to 3·56	−232 −205
	3·56 to 5·86	+193 +192
C diminished to 0, and raised to 3·56.	3·56 to 5·86	+230 +197 +195
	5·86 to 3·56	−209 −205
C increased to 4·86, and diminished to 3·13.	3·13 to 4·86	+149 +149
	4·86 to 3·13	−151 −152
C increased to +13, and diminished to 4·86.	4·86 to 3·13	−180 −157 −157
	3·13 to 4·86	+153 +152
C diminished to 0, and increased to 3·13.	3·13 to 4·86	+189 +153 +153
	4·86 to 3·13	−162 −159

The swings succeeding the first are seen to be all very nearly equal. The negative swings are slightly greater than the positive ones, because the current flowing is smaller, the resistances in circuit larger, and the time-constant of discharge smaller. Although the amount discharged is the same for the two cases, the swing which measures it will be greater in that case for which the time-constant is the smaller.

13. Evidence of hysteresis was obtained in a very marked way from a 2 H.P. transformer lent to Professor Ayrton by Mr. Kapp. The coefficient of self-induction of the primary coil was measured when different currents were traversing the secondary. The magnetizing current (denoted by A) was obtained from accumulators, and varied up to 10 amperes. It was sent in both directions, and its value was read by an Ayrton and Perry ammeter. The coefficient was obtained either by comparison with a standard condenser by the swing-method already described or by the secohmmeter. The test-current used to obtain the coefficient was ·037 ampere for the swing method and ·01 ampere for the secohmmeter method. In the former case means were provided for sending the test-current round the primary in either direction at will, so that the swings obtained corresponded either with the slope PA (see fig. 1) or with the slope PB. If

the total current through the transformer-coils were alternated between two values corresponding with the points A and C, the slope AC was found not only to be less than the preceding slope PA but greater than the succeeding slopes CD, and in some cases several successively diminishing values were obtained. The coefficients corresponding with the slopes PA, AC, and CD will be respectively denoted by the letters L_p, L_r, and L_c. The values obtained could only be reproduced when the magnetic history of the iron was exactly repeated; and whatever the process of magnetization was, the numbers obtained at corresponding parts of successive cycles gradually diminished until the true cyclic values were obtained.

Thus the values obtained by the swing-method for the progressive coefficient L_p were ·087 secohm or ·192 secohm, according as the value of A had been diminished to zero from 1 ampere or from 6 amperes. When A was 1 ampere, values of L_p could be obtained varying between ·074 and ·130 secohm, according to the previous history, and simultaneously the values of the return coefficient L_r could be varied between ·032 and ·053 secohm. The values of the cyclic coefficient L_c (which were obtained by reversing the test-current several times before taking the swing) were, however, fairly constant, and were ·029 secohm when A was zero and ·016 secohm when A was 6 amperes.

In order to obtain a complete set of values for the coefficients L_p and L^c, it was necessary that the iron should go through the same magnetizing processes before any reading was taken. The magnetizing current was therefore diminished to —10 amperes before each observation, and then increased to the particular value of A at which the value of L was observed.

The annexed table indicates the results obtained by the swing method:—

A.	L_p.	L .
0	·243	·0290
2	·230	·0261
4	·083	·0193
5	……	·0177
6	·016	·0155
8	·013	·0120
10	·012	·0097

The values obtained with diminishing currents (*i. e.* for the curve P′A′) were slightly lower. They were arrived at by increasing the current to +10 amperes and then diminishing it to the value of A, at which L_p was required.

The values of L_c were much easier to obtain than those of L_p, for the slightest variation in the main magnetizing current A was sufficient to render it impossible to obtain L_p until the previous magnetizing processes were gone through again. Accumulators were used to provide a constant E.M.F.; but it was nevertheless found difficult to keep A sufficiently constant, so that the test-swings for L_p were taken as quickly as possible to prevent values intermediate between L_p and L_c being obtained. These considerations partially account for the unsatisfactory nature of the results obtained with the Ferranti field-magnets; for, at the time those experiments were made, the necessity of keeping the current quite steady until the swing measuring L was observed was not realized. The numbers given do not show the slight initial increase in the value of L_p for small increasing currents, but more recent experiments have done so.

The values of L_c were also determined by means of the secohmmeter of Professors Ayrton and Perry. This instrument necessarily only measures coefficients for cycles of magnetism. The value of A was varied from zero to 10 amperes positive, thence to 10 amperes negative, and thence to zero again. The value of each coefficient was determined for three different speeds, and the mean taken. The value of the coefficient appeared to diminish slightly as the speed increased. (This effect has been observed in several cases when the magnetic circuit is wholly or partially composed of iron, and is probably due to magnetic lag.) The coefficients L_c obtained for a complete cycle of current values are given in the four columns of the annexed table. The first two columns give the values obtained respectively for increasing and decreasing positive currents; the last two columns indicate those obtained for increasing and decreasing negative currents.

A.	Values of L_e.			
0	·0231	·0231		·0231
2	·0221	·0193	......	......
4	·0167	·0134	·0194	·0147
6	·0150	......	·0141	·0119
8	......	·0105	......	......
10		·0090	·0099	

The values obtained are slightly lower than those obtained by the swing-method. This is accounted for by the fact that the amplitude of the cycle is less. With the secohmmeter the test-current was only ·01 ampere, while it was ·037 ampere with the former method. The value of L_e will be smaller, the smaller the amplitude of the cycle. The secohmmeter is so sensitive that it would have been easy by means of it to measure L_e for cycles of very small amplitude.

Shape of Current Waves.

14. It is a matter of both theoretical and practical interest to determine the way in which currents change when the impressed electromotive forces are given at every instant of time and when the coefficients of self-induction are given in terms of the currents which are flowing. In calculations concerned with alternating-current problems it is usual to assume that the impressed electromotive forces are pure sine functions of the time, and that the coefficients of induction are constant quantities. These assumptions are more convenient than true. The coefficient of self-induction can be at once deduced from the curve of magnetization, and therefore can no more be expressed as a mathematical function of the current than electromagnetism itself. It therefore appears as if, in the treatment of such problems, graphical methods are to be preferred to analytical ones. If, in a simple circuit, the curve connecting the impressed electromotive force with time be given, together with the curve connecting magnetization with current, it is perfectly easy by purely graphical processes to obtain the curve connecting current with time. For we have

$$RC + \frac{dN}{dt} = E,$$

where E is the impressed electromotive force, N is the number of lines of force enclosed by the circuit, R is the resistance of the circuit, C the current flowing, and t the time at which the different quantities are evaluated.

Now, if we can neglect Foucault currents, or possible magnetic lag, or anything analogous which would make N directly dependent on time, we may put

$$\frac{dN}{dt} = \frac{dN}{dC}\frac{dC}{dt},$$

where $\frac{dN}{dC}$ is the coefficient of self-induction, L. If we put

$$C_0 = \frac{E}{R}, \quad T = \frac{L}{R},$$

we obtain

$$\frac{dC}{dt} = \frac{C_0 - C}{T}.$$

Now C_0 is the value of the current which would be flowing if there were no self-induction; and since E and R are given, it is possible to plot a curve having C_0 for ordinates and time for abscissæ. Moreover, since the curve connecting N with C is given, it is possible by graphical processes to find another connecting $\frac{1}{R}\frac{dN}{dC}$ or T with C. This curve (see Plate IX. fig. 3) should be plotted, with the values of C for ordinates and the values of T as abscissæ. It will be found convenient to plot T in the negative direction. The time-ratio T will be in seconds if L is in secohms and R in ohms. The two curves should of course be plotted to the same scale for current and time.

The construction follows immediately from the equation

$$\frac{dC}{dt} = \frac{C_0 - C}{T},$$

and is as follows:—

Suppose P_1 (see Pl. IX. fig. 3) be the given initial point on the curve connecting C with time. Project it parallel to the axes to T_1 on the curve T and to Q_1 on the curve C_0. Project Q_1 parallel to the axis of abscissæ to R_1 on the current axis. Draw from P_1 a line parallel to R_1T_1, and choose a point P_2 on it not far from P_1. P_2 may be regarded as the next point on the current curve, and the process (which is indicated in Plate IX.) may be repeated to obtain a third point P_3, and so on, until the whole curve connecting current with time is obtained.

15. The curves are very readily drawn on squared paper, and yield some remarkable results. If the impressed electromotive force E and the resistance R are constant quantities, the curve C_0 will be a straight line parallel to the axis of time cutting the current axis in a point R. The inclination of the line joining R to any point T on the curve T will represent the rate at which the current is increasing when it has the value corresponding with the point T. Now, if the self-induction is constant or nearly constant, this rate of increase will be great at first but will continually diminish as time goes on. If, however, the self-induction be very variable, the result may be quite different and dependent to a great extent on the value of C_0. Suppose the self-induction to increase at first, and then to diminish. Unless C_0 be very small, there will now be points of inflexion on the current curve. The current will increase very rapidly at first, slower afterwards, then more rapidly, and will finally attain its maximum slowly. If C_0 be such as to magnetize the iron far beyond saturation, this effect may be very marked, and the time taken for the current to rise to a small fraction of its final amount may exceed that taken to rise through the remainder. The writer is indebted to Prof. Silvanus Thompson for the information that this fact has actually been observed when accumulators have been connected in series with the field-magnets of a dynamo and with an Ayrton and Perry dead-beat ammeter. The needle has been noticed to move very slowly at first, and then with great rapidity through the larger portion of the ultimate deflection. The greater the value of C_0, the quicker will the current attain a given fraction of its final value. This may be roughly accounted for by the fact that the mean value of the coefficient is less the greater the value of the final current.

16. When the electromotive force E is alternating, the coefficient L will not only be variable but two-valued. It will depend not only on the value of the current, but also on whether the current is increasing or diminishing. The curve T will therefore consist of two parts— one for increasing current, and the other for decreasing current. Although E and C_0 may be pure sine functions, C will not be a simple sine

function if the coefficient L varies. Suppose a current curve C′, drawn on the assumption that there is a constant coefficient of self-induction equal to the mean of the different values. The true curve C will, roughly speaking, differ from the curve C′ in rising more rapidly when L is less than the average, and in rising less rapidly when L is more than the average. The general effect of the variation of the coefficient will therefore be to introduce ripples into the current curve. It is noteworthy that these ripples are dependent not so much on the speed as on the value of the current.

17. Plate IX. fig. 4 shows an example worked out on the basis of experiments actually made on the Kapp and Snell transformer already referred to. The resistance of the circuit is for simplicity assumed to be 1 ohm, so that the number which represents the coefficient of self-induction L_p in secohms also represents the time-ratio in seconds. The values of L_p given above were obtained for the primary coil when different currents A were traversing the secondary, and should be plotted, not with the values of A, but with the values of n_2A/n_1, where n_1/n_2 is the ratio of the turns on the primary to those on the secondary. This has not been done, as it merely alters the scale of current. The values of L_p differ slightly for increasing and decreasing currents; this has been disregarded, as in the particular cases taken the result would have been but very little altered. The electromotive force acting in the primary circuit (the secondary being open) is supposed to be a simple sine wave having a period of 0·16 second, and is represented by the curve C_0. The current curve obtained will depend for the first few alternations on the initial circumstances. The curves C_1, C_2, C_3, and C_4 represent the first half-wave for different initial values of the current. Of these, C_3 may be taken as the curve which periodically repeats itself and to which all the others will eventually come. It is not in appearance so markedly different from a sine curve as C_1 and C_2. This is because the impressed electromotive force is not sufficient to produce a current capable of magnetizing the iron beyond the saturation-point, at which the value of the coefficient of self-induction begins to diminish. Otherwise the current curve would have sharp peaks in it, as indicated in the curve C_1. This may have

something to do with the fact that it has been found best not to allow the iron of transformers to be magnetized beyond the saturation-point*.

18. It is usual to assume that the electromotive force given by alternating-current dynamos can be represented by a sine function of the time with sufficient accuracy for practical purposes. In the case of a Ferranti dynamo at the Central Institution, however, the approximation to a sine wave is not very good.

The field-magnets were excited with a small current by means of accumulators. A resistance of 100 ohms was placed in the circuit to reduce the time-constant when the current was made. The armature was used as a test-coil, and was coupled in series with a D'Arsonval galvanometer and suitable resistances. A pointer was attached to the spindle, and a scale to the framework of the machine, so that the phase of the armature could be accurately read. The test swings were taken both on making and on breaking the exciting current, and were found practically identical. The swings were taken for many different phases of the armature for one complete alternation. The results showed that the ordinates of the curve of electromotive force gradually differed from those of the most favourably drawn sine curve to the extent of ±5 per cent.

The values of the maximum ordinates of the E.M.F. curve were also compared for one complete revolution of the armature. Five swings were taken in the neighbourhood of each maximum in order to determine its amount. The curious result came out that these maxima varied alternately between values denoted by the numbers 302 and 333. The eight positive maxima did not vary more than $\frac{1}{3}$ per cent. from the value 333, nor did the eight negative maxima vary more than this from the value 302. Since the area of the negative part of the curve must apparently be equal to that of the positive part, it follows that the curve of E.M.F. must be distinctly different from a sine curve. The result found in this case was not at all that which was looked for. It seemed probable that the values of the different maxima would vary gradually, and only repeat themselves after a complete revolution of the machine. It was expected that the curve of E.M.F. would be of the nature of a compound sine curve, having as one term a sine of large amplitude and a period corresponding with the current alternations, and having as a second term a sine of small amplitude but long period corresponding with the revolutions of the machine. Professor Perry has pointed out that, if the wave of E.M.F. is a sine curve marked with minor ripples caused by the presence of subsidiary sine waves of smaller period, the effect of induction in the circuit is to flatten out the minor ripples so that the current produced is more nearly a true sine wave than the electromotive-force wave which is producing it. This is, however, only true on the assumption that the minor ripples have the shorter period. It is very possible that in some alternating-current dynamos there is, due to some want of symmetry, a ripple of small amplitude but having a long period corresponding with the revolutions of the dynamo. Whenever this occurs, the action of induction is to magnify the minor ripples in comparison with the main alternating wave, and the consequent current curve will, especially at high speeds, differ in a very marked degree from the E.M.F. curve of the machine. If the self-induction is a variable quantity, the current curves obtained will be still more rippled, and these ripples will be present at whatever speed the dynamo is revolved.

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* See Mr. Gisbert Kapp's paper "On Alternate Current-Transformers," Proc. Soc. Tel. Engineers, Feb. 1888.

The well-known pulsations to which electric lights fed with alternating currents are subject seem to suggest that the currents obtained from the dynamos used are much less like sine waves than is generally assumed to be the case. These pulsations are so marked in the case of one important London installation that any quickly moving object, such as a walking-stick swayed rapidly to and fro presents, not a blurred image, as it would do under a continuous light, but several distinctly separate images, as if the light were at times very dim. The pulsations are too slow to be due to the ordinary alternations of the current, but might possibly be in time with the revolutions of the dynamo.

Condenser Discharges.

19. It does not appear to have been noticed that self-induction, although always delaying the rise or fall of currents, may sometimes hasten the discharge of a condenser. The current which discharges a condenser has both to rise and fall. Self-induction in the discharge-circuit delays the rise of current less than the fall, because the potential-difference of the condenser is high when the current increases and low when it diminishes ; the rate of change of current depends, not only on the coefficient of self-induction, but also on the E.M.F. tending to cause the change. The well-known oscillations produced when the self-induction is large are simply due to the fact that the current is kept flowing too long. The general result is that the time of discharge is lessened if the self-induction is not too great.

If a condenser of capacity K, charged to a potential V_0, be discharged by a wire having a resistance R and coefficient of self-induction L, the potential of the condenser at any time t is given by one of the equations

$$V = \frac{V_0}{T_1 - T_2} \left\{ T_1 e^{-\frac{t}{T_1}} - T_2 e^{-\frac{t}{T_2}} \right\},$$

$$V = V_0 e^{-\frac{Rt}{2L}} \frac{\sin(\beta t + \gamma)}{\sin \gamma},$$

according respectively as L is less or greater than $\frac{KR^2}{4}$.

In these equations T_1 and T_2 are the two quantities whose sum is KR and whose product is KL ; T_1 is the larger of the two, but is less than KR.

The values of β and γ are given by the equations

$$\beta^2 = \frac{1}{KL} - \frac{R^2}{4L^2}, \quad \tan \gamma = \frac{2L\beta}{R}.$$

The time taken to discharge is proportional to T_1 or to $\frac{2L}{R}$, according as L is less or greater than $\frac{1}{4}KR^2$. Both these values are less than KR, provided $L < \frac{1}{2}KR^2$; so that the insertion of self-induction up to this value only hastens the discharge. The time will be a minimum when $L = \frac{1}{4}KR^2$;

and in this case $T_1 = \frac{1}{2}KR$. It is therefore possible to halve the time of discharge of a condenser by using a suitable amount of self-induction in the discharge-circuit.

A lightning discharge is essentially the same as that of a condenser, and it therefore seems likely that a small amount of self-induction in a lightning-conductor will only improve it. The value of K may be small, but that of R is large, since it quite possibly includes the resistance of the air-gap between the lightning-conductor and the charged cloud; so that the product KR^2, which determines the allowable value of L, may be quite comparable with the self-induction of iron lightning-rods, and may indeed far exceed it.

Quite recently Dr. Oliver Lodge has shown experimentally that an iron conductor is better than a copper one for high-tension electric discharges, and has suggested that, owing to magnetic lag, iron may have less self-induction for such discharges, than in cases where time is an unimportant factor. The foregoing considerations offer quite a different explanation. Further experiments will no doubt be necessary to settle the question; but in the meanwhile there seems to be no real basis for the idea that self-induction in a lightning-conductor is necessarily a disadvantage.

The writer, in conclusion, desires to thank Professor Ayrton for many valuable suggestions received while the experiments were being carried out.

XXXII. *On a Lecture Experiment for determining the Velocity of Sound.* *By* Prof. A. W. Rücker, *F.R.S.*[*] (Abstract.)

The principle of the arrangement is that used by Fizeau in determining the velocity of light. A vibrating reed is used as the source of sound, and a sensitive flame as receiver.

A long U-shaped tube has its two ends placed near and parallel to the plane of a perforated disk, which is capable of rotating about an axis perpendicular to its own plane. The reed and sensitive flame occupy similar positions on the opposite side of the disk.

On rotating the disk, the sensitive flame flares or is quiescent according as the time taken to travel the length of the tube is an even or an odd multiple of $\frac{I}{2n}$, where I is the time of one revolution, and n the number of holes in the disk.

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[*] Read November 12, 1887.

XXXIII. *On a Form of Polariscope for Researches on the Polarization of the Sky.* *By* R. H. M. Bosanquet, *F.R.A.S.*[*] (Abstract.)

Its chief feature is a compound prism of right- and left-handed quartz, which shows coloured bands with polarized light, whatever be the direction of the plane of polarization. It also forms a very sensitive object for polarimeters.

XXXIV. *On the Analogies of Influence-Machines and Dynamos.* *By* Prof. S. P. Thompson, *D.Sc.*[†] (Abstract.)

The author pointed out that in nearly all influence-machines there are two stationary parts ("inductors") electrified oppositely, which are analogous to the field-magnets of dynamos, and a revolving part carrying "*sectors*," which correspond to the "*sections*" of an armature. To prevent ambiguity, Prof. Thompson proposes to call the inductors "*field-plates*," and the revolving parts as a whole an "*armature*."

In the Wimshurst machine both field-plates and armature rotate, and each act as field-plates and armature alternately.

In the two field-plate influence-machines there are four and sometimes six brushes. Two of these act as potential equalizers, two as field-plate exciters, and the remaining two (if any) are generally placed in the "discharge" or external circuit.

The Holtz machine having only four brushes, two serve the double purpose of potential equalizers and discharge-circuit; and this machine excites itself best when the discharging rods are in contact. In this respect it resembles a series dynamo, which only excites itself when the external circuit is closed, but on opening the circuit (say by inserting an arc-lamp) it produces remarkable effects. So in the Holtz machine, on separating the discharging knobs a shower of sparks results.

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[*] Read November 12, 1887. [†] Read November 26, 1887.

The Toepler machine (made by Voss), having six brushes, resembles a shunt dynamo, and excites itself best on open external circuit. Analogies were traced between Thomson's replenisher and the Griscom motor.

Armatures of influence-machines, as in dynamos, can be divided into Ring, Drum, Disk, and Pole armatures, and examples of each kind were mentioned. The "Clark Gas Lighter" is a good example of a Drum armature, and a diagram showing the internal arrangements was exhibited.

An example of an analogue to the Compound Dynamo was mentioned as existing at Cambridge, in the form of a Holtz machine, believed to have been modified by Clerk Maxwell.

Another analogue with dynamos is found in the displacement of the electric field when the armature is rotated, just as the magnetic field of a dynamo is shifted round in the direction of rotation.

Further analogies were traced between "critical velocity" of dynamos (which depends on the resistances in the circuit), below which they do not excite themselves, and a similar critical velocity of influence-machines; *e. g.* in a Wimshurst or Voss machine, the potential equalizing circuit should have a low resistance if they are to excite themselves readily.

Self-exciting dynamos excite better when the iron is bad and retains the magnetism; and influence-machines excite better when the field-plates are made of paper, or such substance as can well retain a residual charge.

Finally, an apparatus analogous to Thomson's "water-dropping accumulator" was exhibited, in which an electric current was generated by mercury falling down a tube between the poles of a magnet.

Prof. Ayrton pointed out an historical analogy between the invention of the influence-machine and the dynamo; for, as Varley anticipated Thomson in the invention of the self-exciting influence-machine, so Hjorth anticipated Varley, Wheatstone, and Siemens in the invention of the self-exciting dynamo.

Dr. Fleming considered that, just as all influence-machines may be looked upon as descendants of Volta's "Electrophorus," so dynamos are those of Faraday's "Coil and Magnet."

XXXV. *On the Rotation of a Copper Sphere and of Copper Wire Helices when freely suspended in a Magnetic Field.*

By Dr. R. C. SHETTLE*. (Abstract.)

THE author exhibited the apparatus with which his experiments, "On the Supposed New Force," were made, the results of which were published in the 'Electrician,' vol. xix. Dr. Hofford has recently made similar experiments, using brass disks, and his results seem to point to "diamagnetic non-uniformity" of the disks as the cause of the phenomena he observed.

XXXVI. *On the Optical Properties of Phenyl-thio-carbimide.*

By H. G. MADAN†. (Abstract.)

THIS body, derived from aniline, is a colourless liquid, density 1·35, and of high boiling-point, 222° C. The refractive indices for the A and G lines are 1·639 and 1·707 respectively. It is thus seen to be a highly refractive liquid, and to have about the same dispersive power as carbon bisulphide, whilst its use in prisms is unattended by many of the risks and inconveniences experienced with carbon bisulphide. The dispersion at the blue end of the spectrum is very marked. Being less mobile than carbon bisulphide it is less affected by convection-currents. The "refractive equivalent," calculated from its chemical constitution, differs considerably from the observed value, and this difference the author believes due to the presence of the phenyl radical and sulphur atom. A polarizing prism made on Jamin's plan, but using phenyl-thio-carbimide as the liquid, gives a fairly wide angular field (about 25°).

Mr. Hilger stated that there was no great need of liquid prisms now, for very dense flint-glass could be obtained with mean index of about 1·8. Dr. Perkin has recently supplied him with Canada balsam perfectly colourless, and which does not tarnish the polished faces of spar; hence one of the greatest objections to the use of Canada balsam in spar-polarizing prisms has been removed.

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* Read December 10, 1887.

† Read December 10, 1887.

Dr. Gladstone pointed out that the constants for the phenyl radical and for sulphur atoms had been determined, and thought the calculated "refractive equivalent" obtained by including these would be much nearer the observed value than the one given by Mr. Madan.

XXXVII. *On the Optical Demonstration of Electrical Stress.*

By Prof. A. W. RÜCKER, *F.R.S., and* Mr. C. V. BOYS*.

A NUMBER of Lecture-experiments were shown illustrating that electrical stress exists in the dielectric separating two charged bodies. The bodies were placed in carbon bisulphide between two crossed nicols, and on electrifying them by means of a Holtz machine, light passed through the analyzer. Two concentric cylinders gave a black cross on the screen similar to those seen on interposing a plate of some uniaxal crystal; and a model illustrating a charged Leyden jar was shown.

XXXVIII. *On a Compact Form of Reflecting-Galvanometer, Lamp, and Scale.*

By G. L. ADDENBROOKE†. (Abstract.)

THE author exhibited this apparatus, designed as a portable commercial instrument, and also a modified Post-office Wheatstone-bridge. In the galvanometer the coils are easily removed or replaced by others, and loose wires for connecting the coils together are avoided; the required connexions being made through the screws, which secure the coils in position. The suspension is arranged so that the needle can be quickly taken out without breaking the fibre, and replaced by another for ballistic purposes if necessary. A simple liquid damping arrangement is provided. The lamp (a paraffin one) is enclosed in a copper cylinder, which also supports the scale. The height of the lamp and scale can be varied, and the beam of light can be directed at different altitudes by having the lens-tube mounted on pivots, the line joining them passing through the centre of the flame. The whole is very compact and portable.

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* Read January 28, 1888.

† Read March 10, 1888.

[illegible] of the present [illegible] to the use of [illegible] has been removed.

Dr. Gladstone pointed out that the constants for the phenyl group and for sulphur atoms had been determined, and that the calculated "refractive equivalent" obtained by [illegible] would be [illegible] the observed value than the one given by Mr. Madan.

XXVII. On the Optical [illegible]. By Prof. S. P. Thompson, F.R.S., and [illegible]*

A number of [illegible] were shown, showing that [illegible] between two crossed nicols [illegible] by means of a [illegible] light passed through the analyser. [illegible]

XXVIII. On a Compact Form of [illegible] Lamp and Scale. [illegible]†

The author exhibited the [illegible] The height of the lamp and scale can be varied, and the beam of light can be directed at different [illegible] through the centre of the flame. The whole is very compact and portable.

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* Read January [illegible]

† Read [illegible]

PROCEEDINGS

OF

THE PHYSICAL SOCIETY

OF LONDON.

July 1888.

XXXIX. *A Theory concerning the Sudden Loss of the Magnetic Properties of Iron and Nickel at certain Temperatures.* By Herbert Tomlinson, *B.A.*[*]

That very high temperatures destroy both the magnetic susceptibility of iron and its power of retaining magnetism has been known since the days of Queen Elizabeth. Thus Gilbert found that a loadstone and a piece of iron equally lost their power of affecting the magnetic needle when very hot; he also noticed that whilst the magnetic property returned to the iron after it had cooled a little, the magnetic virtue of the loadstone was altogether destroyed. Similar results were obtained by Brugmans, Boyle, Cavallo, Barlow and Bonnycastle, Christie, Ritchie, Erman, Scoresby, Seebeck, and others[†]. Faraday found that a steel magnet lost its permanent magnetism rather suddenly at a temperature a little under the boiling-point of almond oil (the best bitter-almond oil boils at 180° C.); it behaved like soft iron till it was raised to an orange-red heat, and then lost its magnetic susceptibility and became indifferent. The temperature at which the retentive power for permanent magnetism was lost appeared to vary in steel with the hardness and structure; in fragments of loadstone it was very high; they retained their permanent magnetism until just below visible ignition in the dark (this would be somewhere about 500° C.), but lost their susceptibility at a much lower temperature than the steel. Nickel was found to lose its susceptibility about 330° C. to 340° C. (according to Becquerel about 400° C.; according to Pouillet 350° C.). Gore has published accounts of experiments relating to the sudden loss of magnetic properties of both iron and nickel at certain temperatures*. Chrystal†, in the course of some experiments on the magnetic sounds in iron, nickel, and cobalt when traversed by an interrupted current of electricity, found evidence of sudden changes in the magnetic susceptibility of iron and nickel at certain temperatures, and gives some curves which enable us to trace the connexion between these changes and the equally curious changes of thermoelectrical power observed by Tait in iron and nickel at certain temperatures ‡. Baur § and Wassmuth ‖ have also taken up the matter. According to the former, for small magnetizing forces the susceptibility of iron at first increases rapidly as the temperature increases, reaches a maximum at red-heat, and then falls suddenly to zero. For large forces the susceptibility decreases gradually up to red-heat, and then falls suddenly to a very small value. According to him, if a bar be cooled from a white heat the first traces of susceptibility are observed at a very bright red, the brighter the greater the magnetizing force. Berson ¶ has published a curve showing the relation between the magnetic induction of nickel and the temperature right up to the point at which the susceptibility practically vanishes, namely, 336° C. The author** has also quite recently published similar curves for nickel, and finds that the susceptibility practically vanishes at temperatures ranging from 333° C. to 412° C. according to the strength of the magnetizing force, the greater the force the higher the temperature. The author, like Berson, finds the temperature at which the nickel *begins* to rapidly lose its temporary susceptibility to be 300° C. Ledeboer[*] has published similar curves for iron. He makes the susceptibility of iron to vanish at temperatures ranging from 750° C. to 770° C., the higher the greater the magnetizing force. The temperature at which the iron *begins* to suddenly lose its temporary magnetic susceptibility is about 680° C.

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[*] Read April 28, 1888.

[†] See Professor Chrystal's Article on Magnetism, Enc. Brit.

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* Proc. R. Soc. vol. xvii. p. 261 (1868–1869); Phil Mag. vol. xxxviii. p. 60 (1869); Phil. Mag. vol. xl. p. 175 (1870). For extracts see 'Electrician,' vol. xx. Nos. 505, 506.

† 'Nature,' vol. xxii. (1880).

‡ Heat, pp. 176–178; Trans. R.S.E. (1873).

§ Wied. *Ann.* xi. (1880).

‖ *Wien. Ber.* (1880, 1881, 1882).

¶ *Ann. de Phys. et de Chim.* vol. vii. (1886).

** Phil. Mag. vol. xxv. No. 156 (1888).

The author[†], in a paper on the recalescence of iron, was led by his experiments to express a strong conviction that with iron there are two critical temperatures at which profound changes take place in the molecular architecture of the metal and at which heat becomes latent. The lower of these critical temperatures he fixes at 550° C. and the higher at 1000° C. Previously to this, but unknown to the author, Pionchon[‡], by most carefully conducted calorimetric researches, had also found in iron two critical temperatures at which heat becomes latent, namely, 660°C.–720°C. and 1000° C.–1050° C. He fixes the value of the latent heat at the lower temperature at 5·3 (about $\frac{1}{15}$th of the latent heat of ice), and that at the higher temperature at 6·0. Pionchon also expresses the specific heat of iron by the following formulæ, in which γ is the specific heat at the temperature t° C.

0° C. to 660° C. $\gamma = 0{\cdot}11012 + 0{\cdot}00005066\,t + 0{\cdot}000000164\,t^2$.
660° to 720° $\gamma = 0{\cdot}57803 - 0{\cdot}002872\,t + 0{\cdot}000003585\,t^2$.
720° to 1000° $\gamma = 0{\cdot}218$.
1050° to 1200° $\gamma = 0{\cdot}19887$.

The author now believes that the lower of the two critical temperatures quoted by him previously, and deduced from his own experiments on the internal friction of iron, may be a third critical temperature[§], and not, as heat first supposed, the temperature at which iron begins to suddenly lose its magnetic properties, which last seems beyond a doubt to be Pionchon's first critical temperature.

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[*] *La Lumière Electrique*, tome xxvii. no. 2.

[†] *Ante*, p. 10.

[‡] *Ann. de Phys. et de Chim.* t. xi. (1887).

[§] If this be so, there is probably another sudden change in the internal friction at 680° C.

In the researches of Moissan[*] on the allotropic modifications of iron oxide two temperatures are very conspicuous namely, 700° C. and 1000° C.

The following table, compiled from the various researches mentioned above, will show at a glance the different critical temperatures of nickel and iron which the author believes may exist[†] :—

Nickel.

A.
- 200° C. Specific heat of electricity changes sign.
- 200°. For low magnetizing forces the magnetic susceptibility begins to rapidly increase.

B.
- 300°. Magnetic susceptibility begins to very rapidly decrease.
- 320°. Specific heat of electricity again changes sign.

Iron.

A.
- 550° C. Specific heat of electricity changes sign.
- 550°. Internal friction of iron begins to rapidly increase.

B.
- 680°. Specific heat of electricity again changes sign.
- 680°. Magnetic susceptibility begins to very rapidly decrease.
- 660°–720°. Heat becomes latent.

C.
- 1000°. Very rapid increase of internal friction.
- 1000°–1050°. Heat becomes latent.
- About 1000°[‡]. Remarkably sudden permanent yielding when under stress, and equally remarkable temporary changes when under stress or strain. Also at this temperature hot iron, which at any temperature is thermoelectrically negative to cold iron, becomes suddenly much more negative.

In the above table the author has not included certain remarkable changes which take place in the electrical conductivity of nickel and iron*, because these do not appear to be sufficiently near any of the above critical points. Thus Smith and MacGregor make the electrical conductivity of pure nickel foil to change suddenly at 149° C., a temperature considerably below 200° C.

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[*] *Ann. de Phys. et de Chim.* t. xxi.

[†] The changes taking place at A may, however, be due to approach of the critical temperature B.

[‡] At least as judged by the colour.

Neither has the author included certain remarkable changes which have been observed to occur in the thermal expansion of steel at high temperatures †, because in the first place there are no such remarkable changes in *iron* ‡, and in the second the critical temperature in the thermal expansion of steel varies very considerably with different specimens; thus in three specimens we have abrupt changes at the temperatures 660° C., 680° C., and 725° C. The author has himself examined the thermal expansion of *iron* at different temperatures right up to and a little beyond that at which the metal showed very conspicuously sudden loss of magnetic properties without at the same time detecting anything like a sudden change in its thermal expansibility.

Evidently the critical points A and B, though occurring at a much lower temperature in nickel than in iron, correspond to similar molecular changes in the two metals, but the third critical point C in iron has apparently no corresponding point in nickel. There are at any rate no such sudden changes in nickel when under stress and strain as occur in iron.

It is, however, the critical point B in nickel and in iron which concerns the present inquiry. Why does the temporary magnetic susceptibility of both these metals diminish so rapidly that, if an iron or nickel wire be surrounded by a magnetizing solenoid always kept in action, and this again be concentric with a secondary solenoid connected with a galvanometer, a very sensible induced current can be observed at the critical temperature both on heating and cooling §? This question is a very important one, and if we can answer it correctly much light will doubtless be thrown upon the nature of magnetism.

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* Macfarlane, Proc. R. S. E. 1875; C. M. Smith and J. G. MacGregor, Proc. R. S. E. 1875, 1876.

† Nouel, *Génie civil*, April 1887.

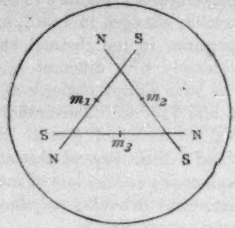
‡ In the curve showing the relation between the thermal expansion of iron and the temperature there is a very slight inflection at 400° C.

§ Even when the wire is only under the influence of the vertical component of the earth's magnetic force, the currents induced when the magnetic properties are lost or restored are quite sensible.

According to the author's view of the matter each molecule of iron, say, contains within itself several magnetic molecules (it may contain several hundreds or thousands).

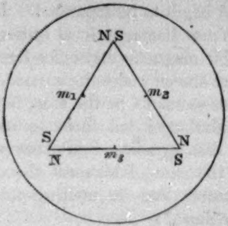
Let the circle in fig. 1 represent a molecule of iron, and let m_1, m_2, and m_3 be the centres of the axes of three magnetic molecules. Each of these magnetic molecules, like the molecule of iron itself, is capable of motion of rotation and of motion of translation. At ordinary temperatures the magnetic molecules have their centres m_1, m_2, m_3 *so close together* that though they are endeavouring to form a *closed magnetic circuit* they are unable to do so. If now we apply a magnetizing force in any direction, each magnetic molecule will be rotated about its axis, and the whole iron molecule will be magnetized and rotated about its axis. When the magnetizing force is removed both the magnetic molecules and the iron molecules will return more or less completely to their original positions. On raising the temperature the centres of both the iron molecules and the magnetic molecules will increase their distances from each other until a certain temperature is reached, when the opposite poles of adjacent magnetic molecules will come into such close proximity that they will be able to rush together so that the system of magnetic molecules forms a close magnetic circuit, as in fig. 2. When the magnetic molecules are in this position a small magnetizing force in any direction will have no appreciable effect; but by employing larger and larger magnetizing force we may obtain

Fig. 1.



sensible evidence of magnetization, as indeed Faraday, Baur, and the author have found. The suddenness of the loss of magnetic properties is easily comprehended when we remember the rapidity with which magnetic attractions increase as the distance between magnetic poles diminishes. We can also understand that heat at this point should become latent, for it is evident that the sudden bringing together of N and S poles, which takes place on heating, will cause a sudden expansion of m_1, m_2, and m_3. Since the action goes on *inside* the iron molecule there may be no sensible sudden increase of distance between the centre of one *iron molecule* and another, and no sensible change in the molar dimensions of the iron. This agrees with the fact ascertained by M. Nouel and the author that, at any rate in the case of annealed iron, there is no sudden change in the thermal expansibility at this critical temperature.

Fig. 2.



When the temperature, after it has been raised to the critical point, is again lowered, the centres m_1, m_2, m_3, will creep closer together again and the magnetic properties will reappear as suddenly* as they were lost.

According to the theory, it would seem that if we could make the magnetic molecules assume a sufficiently closed magnetic circuit, iron and nickel might be converted into diamagnetics. Indeed we can understand that diamagnetics may be such in consequence either of a paucity of magnetic molecules in each molecule of the substance, or from having their magnetic molecules forming nearly closed circuits.

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* The suddenness of both loss and gain of magnetic susceptibility may also be conditioned by internal friction.

Again, if the temperature be raised high enough it would seem possible to partly restore the lost magnetic susceptibility; for then, by the further expansion of m_1, m_2, and m_3, the N and S poles would begin to be separated. If, therefore, such metals as bismuth are diamagnetic at ordinary temperatures, partly because their magnetic molecules form closed circuits, rise of temperature should make them more magnetic or less diamagnetic. This seems to be the case, for the experiments of Plücker and Matteucci led them to conclude that the susceptibility of diamagnetics diminishes with increase of temperature; in the case of bismuth the decrease between ordinary temperatures and its melting-point is said to be about one sixth or more*.

The following experiments were made with the apparatus which has already been described †. The wire to be examined was suspended vertically in the axis of a glass tube, being clamped at its upper extremity into a brass block resting on a wooden support fitting on to the top of the tube, and provided with a terminal screw for making connexion with one pole of a battery of thirty Grove's cells. The current from this battery was employed to heat the wire and passed in or out of the latter through the intermediation of a mercury-cup, into which dipped a stout copper wire attached to a brass block clamped to the lower extremity of the wire.

Experiment I.

A piece of very soft and well-annealed iron wire, 30 cm. long and 1 mm. in diameter, was suspended in the glass tube, so that it could freely swing torsionally. The glass tube was placed inside a magnetizing solenoid, which was in turn coaxial with a secondary solenoid connected with a Thomson's reflecting-galvanometer. The magnetizing solenoid was actuated by 2 Grove's cells, which were kept in action throughout the experiment. As soon as the current from the 30 Grove's cells had heated the wire to a dull red, there was a slight deflection of the Thomson's galvanometer, indicating that the iron had suddenly lost part of its magnetic susceptibility; then, after a pause of three or four seconds, there was a much greater deflection of the galvanometer in the same direction. Again, when the wire was allowed to cool, there was first a slight deflection of the galvanometer, indicating partial restoration of magnetic susceptibility followed immediately by a much larger deflection*. It seems likely that the interval between one deflection and the next, when the wire is being heated or is cooling, is the first of the two periods during which, according to Pionchon, heat is becoming latent. There is a similar pause, though not quite so marked, in the sudden jerk which occurs in an iron wire which has been permanently strained when the critical temperature C has been reached †, and when, according to Pionchon, heat a second time becomes latent.

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* Chrystal, Enc. Brit., Art. Magnetism.

† Phil. Mag. vol. xxiv. p. 258.

Experiment II.

In this experiment an attempt was made to abolish the deflections of the Thomson's galvanometer, due to the sudden loss or restoration of the magnetic susceptibility, by torsionally oscillating the wire during the whole period of heating or cooling. But no amount of torsional oscillation availed anything.

Experiment III.

The experiments of Faraday and others, quoted previously, show that there is a temperature at which *permanent* magnetization in steel begins to be rather suddenly lost, and that this temperature is considerably lower than the temperature at which the temporary magnetic susceptibility begins to rapidly diminish. The author has also found that when a permanently twisted wire is heated it begins at a certain temperature to rapidly ‡ lose its permanent strain. It seemed, therefore, desirable to ascertain if this sudden loss of permanent molecular strain would, like permanent magnetization, take place at a lower temperature than the loss of temporary susceptibility. The iron wire in the last experiment was, therefore, permanently twisted through twenty revolutions and then released from torsional stress. When, on heating, a certain temperature had been reached there was a sudden untwisting of the wire, *followed after an interval of several seconds* by the usual deflection of the Thomson's galvanometer, which showed that the temporary magnetic susceptibility was beginning to disappear. This experiment was repeated at least six times with different pieces of the same kind of wire, and, invariably, with the same result—the permanent torsion, like the permanent magnetism, began to disappear suddenly at a temperature considerably below the temperature at which the temporary magnetic susceptibility began to disappear.

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* This pause between one deflection and another has already been noticed by Mr. Newall (Phil. Mag. vol. xxiv. p. 435).

† Phil. Mag. vol. xxiv., 1887.

‡ This sudden loss of permanent strain must not be confounded with the sudden jerk which takes place at the critical temperature C.

The contrary, however, was the case with a specimen of annealed nickel wire* of the same length as the iron but of rather less diameter. When this wire had received a few turns of permanent torsion†, and was afterwards heated, the loss of temporary magnetic susceptibility occurred at a much lower temperature than the rapid loss of permanent torsion; whilst Berson has shown that the permanent *magnetization* of nickel resembles the permanent magnetization of iron in disappearing before the temporary susceptibility.

This experiment, therefore, shows that the temperature at which permanent magnetism begins to suddenly disappear is not the temperature at which permanent torsion begins to suddenly disappear.

XL. *On Electromotive Forces of Contact.*

By Charles V. Burton, *B.Sc.*‡

The principal object of this paper is to discuss the seats of the electromotive forces developed by the contact of conductors, and by considering the transformations of energy which occur when two insulated conductors are brought into contact, an attempt is made to give a logical demonstration of some of the views put forward. In the latter part of the paper a description is given of some experiments on the nature of the electrification of non-conductors by contact or friction.

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* Containing nearly 98 per cent. of nickel

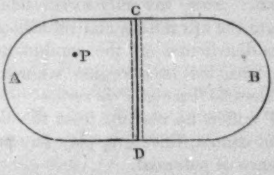
† Only two or three turns can be given before the wire breaks.

‡ Read April 28, 1888.

When two conductors are brought into contact, an electromotive force (E) is set up between them, which drives electricity across the junction, until the difference of their potentials* has an E.M.F. equal and opposite to E. I shall assume that E is always the same so long as we keep to the same substances and the same temperature; and also that when electric equilibrium has been established each conductor is at a uniform potential throughout, except very near the junction, and that there the potential changes in general by a finite amount as we pass through a layer of very small but finite thickness, which includes the surface of separation.

Let the two conductors A and B (fig. 1) be in contact over the surface CD, and let the difference of their potentials be finite. The potential is constant throughout the extent of A, and also throughout the extent of B, except within a very thin layer including the surface CD, which may be called the *layer of variable potential*, and where the potential may be supposed to change continuously from that of A to that of B as we pass from one side of the layer to the other. Now, consider the distribution of electricity on the system. Electric masses will be distributed on the external surfaces of A and B, but not within these surfaces, except within the layer of variable potential. Here the distribution evidently depends upon the thickness of the layer, the law of variation of

Fig. 1.



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[*] Wherever the potential of a conductor is spoken of, the potential at any point throughout its volume is intended, and not the potential at a very near external point. I must apologise for this unusual use of the term.

potential across the layer, and the specific inductive capacity of A and B. This last quantity is evidently the true or finite specific inductive capacity, and not the apparent or infinite specific inductive capacity which conductors appear to have in ordinary electrostatic experiments; for in the latter case the electric potential energy of the layer CD would be infinite.

It is usually supposed that the contact E.M.F. between two conductors is due to some molecular action between them. And my first point is to show that this action is confined to molecules within the layer of variable potential.

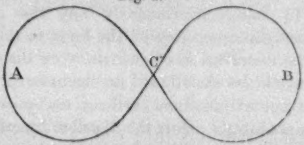
For consider a particle P (fig. 1) within the conductor A, so that all around P the potential is constant. Then if P takes part in any way in maintaining a difference of potentials between A and B, it must in turn be reacted on by the electric forces which tend to equalize the potential throughout the system. Now, the only forces tending to equalize the potential are the attractions and repulsions *inter se* of the electric masses distributed on the conductors; and the particle P being situated in a region where the potential is constant, the electric force at P is zero.

Therefore P suffers no reaction from the electric masses of the system and cannot, therefore, play any part in maintaining the difference of potential.

Hence *the molecular action which gives rise to a contact E.M.F. between two conductors is confined to the immediate neighbourhood of the junction.*

Now, suppose that two conductors A and B (fig. 2), whose contact-difference of potentials is P, are originally at the same potential, and are then brought into contact at C. As soon as this is done an E.M.F. E (= −P) will act between A and B, and will drive electricity across the junction at C, until the difference of potentials of A and B becomes equal to P.

Fig. 2.



At the beginning of the operation, when contact first occurs, there is no E.M.F. opposed to E, so that when E drives a quantity dM of electricity across the junction, the work done is EdM. As there is no opposing E.M.F. to be overcome, the whole of this work is spent in producing heat, according to the Joule effect.

At a later stage, when the difference of potentials has become (say) p, the total E.M.F. between the conductors $=\mathrm{E}+p$; p being of course of the *opposite sign* to E. Hence, when the quantity dM crosses the junction, the work $(\mathrm{E}+p)d\mathrm{M}$ is spent in producing the Joule effect ; while the electric potential energy of the system increases by $-pd\mathrm{M}$; so that the total work done on dM by the E.M.F. $\mathrm{E}=\mathrm{E}d\mathrm{M}$.

Hence, if in order to establish the difference of potentials P between A and B, the quantity M of electricity must cross the junction, the work done by the E.M.F. E during the operation is E.M., half this work being spent in producing the Joule effect, and half in raising the electrostatic potential energy of the system.

But the E.M.F. E is due to molecular action in the immediate neighbourhood of the junction, and, therefore, when the E.M.F. E does work by causing a displacement of electricity, a corresponding amount of molecular energy is absorbed *at the junction*.

Now, E being finite, suppose that the surface of contact at the junction C (fig. 2) is very small, and that the electrostatic capacities of the conductors are very great. Then M will be very great, and so will EM. In fact, keeping the junction as small as we please, we may increase EM indefinitely by increasing the electrostatic capacity of the system.

But, by hypothesis, the E.M.F. E will always be maintained so long as we keep to the same substances, and maintain them at the same constant temperature.

Hence, when the E.M.F. E does work, the molecular energy absorbed is of such a nature that it can be supplied in indefinite amount by a small finite junction maintained at a constant temperature.

There are only two kinds of energy which fulfil this condition:—(1) Heat; (2) Chemical action at the junction.

First, suppose that the conductors A and B (fig. 2) are incapable of acting chemically upon one another. Then for every quantity dM of electricity which crosses the junction in the direction of the E.M.F. E, this E.M.F. does work EdM, and an equivalent amount of heat is absorbed at the junction. And, conversely, if electricity dM cross the junction in the opposite direction, work will be done against those molecular agencies at the junction which maintain the E.M.F. E, *i. e.* the work done on the E.M.F. E will appear in the form of heat.

Hence, *the true contact E.M.F. between two chemically inactive conductors is equal to their coefficient of the Peltier effect expressed in absolute measure.*

An argument sometimes advanced against this proposition is substantially as follows (I quote from Professor Lodge's paper in the Phil. Mag. for April 1885, p. 260) :—" When Q units of electricity are transmitted against a force E, work EQ is done; also when they are transmitted up a difference of potential V^1-V, work $Q(V^1-V)$ is done; but in an open circuit containing an electromotive junction, V^1-V is produced by and is equal to E. Hence, at an electromotive junction no work need be done by a current; in other words, the existence or non-existence of a Peltier effect has nothing to do with the existence or non-existence of a local E.M.F." *

But although on an open circuit, there are two equal and opposite E.M.F.'s at the junction, and consequently there is no resistance to the flow of electricity in either direction, still the E.M.F. which acts in the direction of the flow *will do work upon the opposing E.M.F.*

A mechanical example may make this clearer. If a particle be acted upon by two equal and opposite forces, each of one dyne, the smallest possible force will be able to displace the particle in any direction; but if the particle be displaced one centimetre in the direction of one of the dyne-forces, this force will do just one erg of work upon the other force.

Now, at the junction between two chemically inactive conductors, the two E.M.F.'s are (1) an E.M.F. E due to a tendency to absorption of heat-energy at the junction, and transformation of the absorbed energy into electrical energy by displacement of electricity across the junction, and (2) an E.M.F. P (= −E) due to difference of potentials between the conductors.

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* Professor Lodge refutes this by a hydrostatic analogy.

Hence, if electricity M cross the junction in the direction of the E.M.F. E, heat-energy EM is absorbed in order to drive the quantity M up the step of potential; while if electricity M cross the junction in the opposite direction, energy EM will be given out in descending the step of potential and will appear in the form of heat.

Returning now to the consideration of the conductors A and B (fig. 2), suppose that they are capable of acting chemically upon one another. In this case the energy required to establish the difference of potentials between A and B may be supplied at the junction either in the form of heat absorbed there or chemical action taking place. For the sake of simplicity let the Peltier E.M.F. be zero, so that we have only to deal with a chemical E.M.F. It will also be simplest to consider one of the conductors to be a solid and the other a liquid, so that any chemical action which may take place will not modify the nature of the subtances in contact.

For every quantity M of electricity which crosses the junction in the direction of the E.M.F. chemical energy EM will be absorbed, *i. e.* an amount of chemical action proportional to M will take place.

This is in accordance with Faraday's Electrolytic Law.

Professor Lodge considers that the E.M.F. between chemically active substances is due to a *tendency* to chemical action. But mere tendency to chemical action at a finite junction could not be the source of an indefinite amount of energy, and could not, therefore, account for the phenomena in the case discussed above. There must be actual chemical action. It appears in fact (excluding all idea of a Peltier E.M.F.) that when two chemically active conductors are brought into contact, electricity in general crosses the junction, and establishes a difference of potential between them, and *the amount of chemical action which takes place is precisely the equivalent of the electricity which crosses the junction determined in accordance with Faraday's Electrolytic Law.*

Let K = number of absolute units of energy evolved when one gram of (say) the substance A enters into the kind of combination which takes place in the case considered.

Let k=number of grams of A which enters into combination per unit flow of electricity.

Then for every unit of electricity which crosses the junction, E units of chemical energy are absorbed ; ∴ E/K grams of A are dissolved.

$$\therefore\ E/K=k \quad \text{or} \quad E=kK.$$

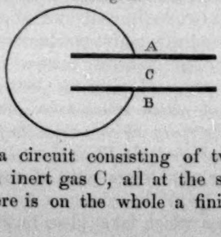
That is, *the E.M.F. is equal to the energy of combination of one electrochemical equivalent.*

Of course this is only the case where all the energy of the chemical action is converted into electrical energy ; as in the case of pure zinc dipped in dilute sulphuric acid. From this it follows that in the case of the typical cell, for instance, where pure zinc and pure copper are immersed in dilute sulphuric acid, the principal E.M.F is at the contact of the zinc and acid, an opposing E.M.F. being set up at the contact of acid and copper.

If we construct a cell consisting of zinc and carbon dipping in dilute H_2SO_4, there will be no power of chemical action at the liquid-carbon junction, so that the only E.M.F. there is a Peltier E.M.F. Neglecting Peltier E.M.F.'s altogether, the E.M.F. of such a cell would be equal to that of the zinc-liquid junction.

The apparent contact E.M.F.'s of metals, as measured in air, must be due chiefly to the air-metal contacts, since a metal-metal contact can only be the seat of a Peltier E.M.F. It also appears that if the apparent E.M.F. between two metals were measured inductively in some chemically inactive gas (or liquid), the result would be the sum of three Peltier E.M.F.'s, and would probably be small. There even seems to be some reason for supposing it to be zero.

Fig. 3.



For consider a circuit consisting of two metals A and B (fig. 3) and an inert gas C, all at the same temperature. Suppose that there is on the whole a finite E.M.F. in the circuit ACBA, *i. e.* that the potential of the gas C varies as we pass from the plate A to the plate B. Now, it is evident that if the gas C has any trace of conductivity, or any power whatever of equalizing potentials by convection, a feeble current will flow continuously around the circuit ACBA, and will derive its energy from absorption of heat at one or more of the junctions, which would be a violation of the Second Law of Thermodynamics. If the metal A were furnished with points as fine as possible, and if these points were brought within an extremely small distance of B, they would greatly favour any conductive or convective tendency that might exist; and if by such an arrangement a convective discharge of only one molecule of electricity in a century were established, it would constitute a violation of the Second Law of Thermodynamics.

If we admit that in an inert gas C the difference of potential between the layers in contact with A and B is finite, we must assume that the gas is an *absolutely perfect insulator* for such an E.M.F. Even if this property is assumed it does not follow that Volta's Law is inapplicable to the case of gases and metals.

I have endeavoured to show how a Peltier E.M.F. depends on a tendency to absorb heat-energy and convert it into electrical energy. Now from the law of successive contacts of chemically inactive conductors, it is evident that, the temperature being fixed, we can connect with each substance a constant (called its "Peltier-constant") such that the E.M.F. between any two inactive conductors is equal to the difference of their respective constants, which is only another way of stating Volta's Law.

Now it seems not unlikely that the Peltier constant of a given conducting substance, at a given temperature, depends only on the size, form, &c. of its molecules, and the nature of their heat-motion; and, if so, each non-conductor should also have its Peltier constant, since the conditions are essentially the same in the two cases.

Thus the law of successive contacts might hold for chemically inactive non-conductors as well as for conductors.

It might even be that the law is universally true so far as Peltier E.M.F.'s only are concerned; but of course these are only speculations.

The following are some analogous properties of Peltier E.M.F.'s and chemical E.M.F.'s arranged respectively in parallel columns :—

PELTIER E.M.F.	CHEMICAL E.M.F.
1*a*. When the two substances originally at the same potential are brought into contact an E.M.F. E_1 is established across the junction; this E.M.F. continues to displace electricity across the junction until the E.M.F. due to difference of potentials $= -E_1$	1*b*. When the two substances originally at the same potential are brought into contact an E.M.F. E_2 is established across the junction; this E.M.F. continues to displace electricity across the junction until the E.M.F. due to difference of potentials $= -E_2$.
2*a*. The energy required to establish the difference of potentials is supplied by heat absorbed at the junction.	2*b*. The energy required to establish the difference of potentials is supplied by chemical action taking place at the junction.
3*a*. For every quantity M of electricity which crosses the junction in the direction of the E.M.F. E_1, a quantity of heat-energy E_1M is absorbed at the junction.	3*b*. For every quantity M of electricity which crosses the junction in the direction of the E.M.F. E_2, a quantity of chemical energy E_2M is absorbed at the junction.
4*a*. For every quantity M of electricity which crosses the junction against the E.M.F. E_1, heat-energy E_1M is given out at the junction.	4*b*. For every quantity M of electricity which crosses the junction against the E.M.F. E_2, energy E_2M is given out at the junction, and appears either as chemical-energy (electrolysis) or as heat.
5*a*. The heat absorbed at the junction during the operation (1*a*) is the equivalent of the electricity which crosses the junction, determined in accordance with the laws of the Peltier effect.	5*b*. The chemical action at the junction during the operation (1*b*) is the equivalent of the electricity which crosses the junction, determined in accordance with Faraday's Electrolytic Law.

Sir W. Thomson has deduced a lower limit to the size of atoms by a method which is well known. Professor Lodge points out that the limit cannot be assigned with certainty, until the true contact-difference of potentials of the substances concerned is known. But there is another point to be considered. The method assumes that there is a kind of affinity between the molecules of the two metals concerned ; that on contact being made, this affinity gives rise to an E.M.F. which establishes a difference of potential, the necessary energy being furnished by the partial satisfaction of the said affinity. The method further assumes that when the metals are fused together it is this same affinity which, being satisfied as completely as possible, gives rise to an evolution of heat. But if (as appears from what precedes) the energy required to produce the difference of potentials is supplied by absorption of heat at the junction, and if the contact E.M.F. is due to a tendency to convert heat-energy into electrical energy, then the heat evolved on fusing the metals together can have no direct connexion with E.M.F.'s of contact, and the method would not apply to the case of two metals.

It would, however, obviously apply where the E.M.F. was due to chemical affinity, as in the case of zinc and oxygen proposed by Professor Lodge (Phil. Mag. May 1885, p. 363). In all these investigations the electrostatic capacities are calculated on the assumption that the specific inductive capacity of the conductors is unity. I have not been able to find any experimental justification for this assumption.

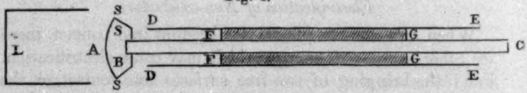
Electrification of Non-conductors.

When two non-conductors are brought into contact, there are other sources of energy which may cause electrification. For if the bringing of two free surfaces into contact, or the striking or rubbing of the bodies together, should give rise to electrification, the charges so produced would remain separated, owing to the insulating properties of the substances. This source of electrification is absent in conductors, as any electrification produced between them by sudden violence would be destroyed by conduction, the final difference of potentials between the bodies being always that due to a definite contact E.M.F.

The difference between the two cases may be illustrated by considering the case of a spring. If a force be applied to the spring a definite compression will ensue, supposing that there is no friction; and if by any temporary impulse the compression be increased or diminished, it will regain its former value when the impulse ceases to act. The inverse of the strength of the spring represents the capacity of the system, the force compressing the spring the E.M.F. of contact, the distance through which the spring is compressed the quantity of electricity displaced, and the potential energy of the compressed spring the electrostatic potential energy of the two conductors.

If we suppose compression or extension of the spring to be accompanied by friction, a compression effected by a sudden shock will be maintained. This illustrates what perhaps takes place with non-conductors. In order to test which view is the more correct, I have made a few experiments on the electrification of glass and ebonite when dipped into mercury. This method of experimenting seemed to offer several advantages. The mercury being permanently connected to earth was always at a known potential (zero), and it was only necessary to measure the charge of the ebonite or glass. Then, again, contact was obtained over the whole surface submerged, with very little mechanical violence when desired; and the conditions of contact could be considerably varied by using more or less suddenness and violence and longer or shorter duration of contact.

Fig. 4.



The ebonite or glass employed consisted of a small circular slab S (fig. 4) with a convex surface A, which was attached to one end of a rod BC of the same material. This rod was encased in a brass tube DE, inside which it was fastened by means of packing FG. The length BF of ebonite or glass served to insulate the slab A. In the case of the glass the length BF was varnished. The charge on A was measured by means of the conductor L connected to a Thomson electrometer. A was placed near to L, which was put to earth and then insulated. On withdrawing A, a deflection was produced proportional to the charge on A. The movement of A was controlled by making the tube DE slide in metal bearings between fixed stops. The tube DE being of metal, no electrification was produced by friction in the bearings. The following results were obtained:—

(a) *Ebonite and Mercury.*	DEFLECTION.
Dipped once and quickly withdrawn	+10 to +12
" " very quickly withdrawn	+15
" for 10″ quickly withdrawn	+ 7 to + 15
" " 15″ " "	+ 6 to + 10
" " 30″ " "	+ 6 to + 13
" " 60″ " "	+ 5 to + 9
30 rapid splashes in the mercury	− 6
50 not very rapid splashes in the mercury	+ 7
100 rapid " " "	−11
200 " " " "	−10

(b) *Glass and Mercury.*	DEFLECTION.
Dipped once quietly	+ 6 to + 10
20 rapid splashes	− 7
50 " "	− 1
60 " "	− 6
90 " "	+ 4
100 " "	− 3

The above results, it will be seen, are very irregular, owing no doubt to the fact that all the circumstances of contact could not be accurately known. These irregularities, and especially the variable sign of the electrification, make it clear that the effects are not due to a true and definite contact E.M.F., but are dependent in some degree on the manner in which the surfaces are brought together and separated. The experiment of Canton in 1753 (where the rough and polished parts of a glass tube became oppositely electrified when rubbed with the same cloth) seems to point in the same direction.

Professors Ayrton, Schuster, S. P. Thompson, and J. Perry discussed the points raised, and it was considered that direct experiment on contact electromotive force in a very perfect vacuum could alone decide the question.

This experiment seems almost as impracticable as a direct measurement of the size of a molecule. Yet the two questions may be equally capable of solution by inductive reasoning. The question is whether the reasoning given above is or is not conclusive.—C. V. B.

XLI. *On Magnetic Lag.*

By Thomas H. Blakesley, *M.A.*[*]

In bringing my views on Transformers before the Physical Society it is my desire to emphasize :—

(1) How the magnetic lag, if it exist, may be measured by employing dynamometers of low resistance.

(2) That the magnetic lag has a real existence.

(3) That the magnetic lag necessarily accompanies an absorption of work involved in the reversal of polarity in the iron, and how this may be measured.

(4) The points in the general argument where scientific facts are wanting, and the direction which investigation should take to meet this want.

The possibility of the existence of a magnetic lag renders the problem a different one from that of two coils acting and reacting upon themselves by means of mutual and self-induction, whose coefficients, being geometrical, are constant.

For the latter problem I gave in the year 1885 a complete solution, but I pointed out that the completeness of the result rested upon the absence of anything in the nature of hysteresis (a word not then in use) or work done in the field.

The following year Mr. George Forbes, F.R.S., gave what should have been (but for the very poor reporting of the Society of Arts' Journal) a solution of the "secondary generator" problem, treating it as a case of two coils, assuming that "the magnetism of the core varies as the sum of the currents in the two coils;" and the same gentleman has treated the subject again in a recent paper before the Society of Telegraph-Engineers and Electricians, in which he makes the same assumption, and says, referring to the harmonic functions which he attributes to the electrical and magnetic quantities involved, that the existence of magnetic hysteresis would cause departure from the harmonic character, but that, being insignificant so long as the magnetic induction in the iron is not high, its consideration may be omitted ; statements which seem rather to evade than to overcome the difficulty.

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[*] Read May 12, 1888.

Mr. Gisbert Kapp, who has done so much good work in the practical development of transformers, also, in my opinion, makes the same assumption, that the state of magnetization in the core coincides with the magnetic stress resulting from compounding the stresses derived from the two coils.

In the view I shall put forward I shall assume :—

(1) That the variations are harmonic.

(2) That the only induction in the secondary coil is derived from the core, and is therefore, as regards phase, in quadrature with the magnetization. As the current in the secondary coil will be considered as producing one of the components of the stress producing magnetization, itself reacting upon the coil, the necessity of introducing a special E.M.F. of self-induction is obviated.

(3) That each turn in either coil embraces the same number of magnetic lines.

I shall also make use of the following symbols :—

E, the maximum electromotive force of the machine ;
I_1, the maximum value of the current in the primary circuit ;
I_2, the maximum value of the current in the secondary circuit ;
$\pi-\theta$, the angle of phase-difference between the currents ;
m, the number of turns of wire in the primary coil ;
n, „ „ „ secondary coil ;
ϕ, the angle of magnetic lag ;
r_1, the resistance in the primary circuit ;
r_2, „ „ secondary circuit ;
$\left.\begin{matrix}\alpha_1\\ \alpha_2\end{matrix}\right\}$ are the readings of two dynamometers placed respectively in the primary and secondary circuits, their constants being A and B, so that $\frac{I_1^2}{2}=A\alpha_1$, $\frac{I_2^2}{2}=B\alpha_2$;
α_3 is the reading of a dynamometer one of whose coils is in the primary, the other in the secondary circuit. Its constant is C.
M is the maximum magnetization.

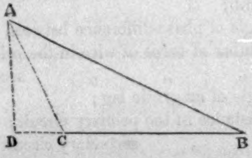
The magnetic stress produced by each coil is proportional to the current in that coil multiplied by the number of turns in the coil, and is here taken to be that product, called very often the ampere-turns. Its maximum value in the primary coil is mI_1 and in the secondary coil it is nI_2.

Now the observations on the dynamometers A and B furnish us with a knowledge of I_1 and I_2 in any case; and m and n are details of the construction of the transformer. Thus we are in possession of the two quantities mI_1 and nI_2.

But the three dynamometer observations enable us to determine the angle of phase-difference between the currents, as I have elsewhere explained :—

$$\cos\theta = \frac{C\alpha_3}{\sqrt{A\alpha_1 B\alpha_2}} \quad \text{for } C\alpha_3 = \frac{I_1 I_2 \cos\theta}{2}.$$

We are therefore in possession of the two components of the magnetic stress and of the angle between them. Hence we are virtually in possession of the whole magnetic stress, and its phase relatively to its components. If the resultant is in quadrature with that component which results from the current in the secondary coil, it is in the same phase as the magnetization, which is in quadrature with that component; but not unless this is the case.



Let the line A B represent mI_1 or the magnetic stress in the primary circuit, and let B C represent the magnetic stress in the secondary, and let A B C be the angle θ, found as above. Then A C is the resultant magnetic stress.

But the magnetization is in quadrature with B C. Draw A D at right angles to B C. Then C A D represents the magnetic lag, which is seen to vanish if A C B is a right angle. The condition of the existence of lag is therefore that

$$CB < AB\cos\theta,$$

which in terms of the dynamometer observations is

$$n\sqrt{2B\alpha_2} < m\sqrt{2A\alpha_1}\,\frac{C\alpha_3}{\sqrt{A\alpha_1\,B\alpha_2}},$$

or

$$B\alpha_2 < \frac{m}{n}\,C\alpha_3.$$

The observation on the dynamometer in the primary is seen to be eliminated. Thus this question can be tested with two dynamometers only. The amount of lag is represented by the angle CAD. We can easily express its tangent in terms of the three dynamometer observations.

$$\tan\phi = \frac{CD}{DA} = \frac{DB-BC}{AB\sin\theta} = \frac{\frac{DB}{AB}-\frac{BC}{AB}}{\sqrt{1-\cos^2\theta}}$$

$$= \frac{\cos\theta - \frac{nI_2}{mI_1}}{\sqrt{1-\cos^2\theta}}$$

$$= \frac{\frac{C\alpha_3}{\sqrt{A\alpha_1\,B\alpha_2}} - \frac{n}{m}\sqrt{\frac{B\alpha_2}{A\alpha_1}}}{\sqrt{1-\frac{C^2\alpha_3^2}{A\alpha_1 B\alpha_2}}}$$

$$= \frac{C\alpha_3 - \frac{n}{m}B\alpha_2}{\sqrt{A\alpha_1\,B\alpha_2 - C^2\alpha_3^2}}.$$

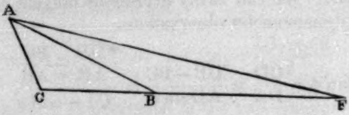
Thus the angle of magnetic lag, if it exist, can be detected with two dynamometers and measured with three.

In dealing yet further with the results furnished by the observations, we must remember that the waxing magnetism has the same inductive effect in producing E.M.F. in each turn of the coils of the two circuits. But we can, from the observation of dynamometer B, say what that E.M.F. per turn is. The whole E.M.F. in the secondary coil is $I_2 r_2$, consequently the E.M.F. per turn is $\frac{I_2 r_2}{n}$. Therefore in the primary the total E.M.F. arising from magnetic induction is $m\frac{I_2 r_2}{n}$.

The current arising from the same source is $\frac{mI_2}{n}\frac{r_2}{r_1}$, and the magnetizing stress on this account is $\frac{m^2}{n}\frac{I_2 r_2}{r_1}$, which must be looked upon as one of the components of the whole magnetizing stress due to the primary current; and this component is in the same phase as the magnetizing stress in the secondary.

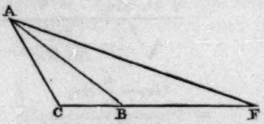
Hence, returning to the figure, if we produce CB to F so that $\mathrm{CB} : \mathrm{BF} :: n\mathrm{I}_2 : \dfrac{m^2}{n}\dfrac{\mathrm{I}_2 r_2}{r_1}$,

$$:: 1 : \frac{m^2}{n^2}\frac{r_2}{r_1}.$$



FB is one of the components of AB.

The other component (*i. e.* that arising from the machine's proper electromotive force) is AF. Hence $\mathrm{AF} = m\dfrac{\mathrm{E}}{r_1}$.



It follows that the electromotive force E, which the machine is exerting, may be thus determined by means of the dynamometer observations.

$$\mathrm{AF}^2 = \mathrm{AB}^2 + \mathrm{BF}^2 + 2\mathrm{AB}\ \mathrm{BF}\cos \mathrm{ABC},$$

$$\frac{m^2\mathrm{E}^2}{r_1^2} = m^2\mathrm{I}_1^2 + \left(\frac{m^2}{n^2}\frac{r_2}{r_1}\right)^2 n^2\mathrm{I}_2^2 + 2m\mathrm{I}_1\frac{m^2}{n^2}\frac{r_2}{r_1}n\mathrm{I}_2\cos\theta,$$

$$\therefore\ \mathrm{E}^2 = r_1^2\mathrm{I}_1^2 + r_2^2\mathrm{I}_2^2\frac{m^2}{n^2} + 2r_1r_2\frac{m}{n}\mathrm{I}_1\mathrm{I}_2\cos\theta,$$

$$= r_1^2\, 2\mathrm{A}\alpha_1 + \frac{m^2}{n^2} r_2^2\, 2\mathrm{B}\alpha_2 + 4r_1r_2\frac{m}{n}\mathrm{C}\alpha_3,$$

$$= 2\left\{ r_1^2\mathrm{A}\alpha_1 + r_2^2\frac{m^2}{n^2}\mathrm{B}\alpha_2 + 2r_1r_2\frac{m}{n}\mathrm{C}\alpha_3 \right\}.$$

Another interesting magnitude is AC, or the total impressed magnetic force.

$$
\begin{aligned}
\mathrm{AC}^2 &= \mathrm{AB}^2+\mathrm{BC}^2-2\mathrm{AB}\,\mathrm{BC}\cos\theta,\\
&= m^2\mathrm{I}_1^2+n^2\mathrm{I}_2^2-2mn\mathrm{I}_1\mathrm{I}_2\cos\theta,\\
&= 2m^2\mathrm{A}\alpha_1+2n^2\mathrm{B}\alpha_2-4mn\mathrm{C}\alpha_3,\\
&= 2\{m^2\mathrm{A}\alpha_1+n^2\mathrm{B}\alpha_2-2mn\mathrm{C}\alpha_3\}.
\end{aligned}
$$

By means of this we may calculate what current should be passed through the primary circuit, the secondary being open, to produce the same state in the core.

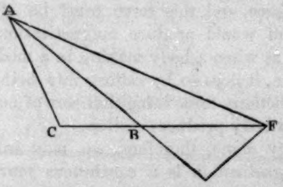
But perhaps the most interesting point to men of science and to civil engineers is the question of power. We may approach it thus perhaps in the simplest way.

By dropping a perpendicular from F upon AB produced we easily see that

$$\mathrm{AF}\cos\mathrm{BAF}=\mathrm{BF}\cos\mathrm{ABC}+\mathrm{AB}.$$

Multiplying through by AB we have

$$\mathrm{AF}\,\mathrm{AB}\cos\mathrm{BAF}=\mathrm{AB}\,\mathrm{BF}\cos\mathrm{ABC}+\mathrm{AB}^2.$$



Substituting the electric quantities for the geometrical,

$$\frac{m\mathrm{E}}{r_1}m\mathrm{I}_1\cos\mathrm{BAF}=m\mathrm{I}_1\frac{m^2}{n^2}\frac{r_2}{r_1}n\mathrm{I}_2\cos\theta+m^2\mathrm{I}_1^2\;;$$

multiply through by $\frac{r_1}{2m^2}$

$$\frac{\mathrm{EI}_1\cos\mathrm{BAF}}{2}=r_2\frac{m}{n}\frac{\mathrm{I}_1\mathrm{I}_2\cos\theta}{2}+\frac{r_1\mathrm{I}_1^2}{2}.$$

But the term on the left is the expression for the total power, and those on the right hand may be expressed in terms of the dynamometer observations.

Thus the total power

$$=r_1\mathrm{A}\alpha_1+r_2\frac{m}{n}\mathrm{C}\alpha_3.$$

The first term here is obviously the power at work heating the primary coil.

$r_2 B\alpha_2$ is as obviously the power heating the secondary coil.

If, therefore, we write the total power

$$=r_1 A\alpha_1 + r_2 B\alpha_2 + r_2\left\{\frac{m}{n} C\alpha_3 - B\alpha_2\right\},$$

we see that the power involved with the magnetic lag is

$$r_2\left\{\frac{m}{n} C\alpha_3 - B\alpha_2\right\},$$

the form showing that it disappears if the lag does so.

Thus we are led to the conclusion that a magnetic lag involves a loss of power, and any loss of power due to molecular action in the core taking place in the course of the alternations of magnetization must necessarily produce lag.

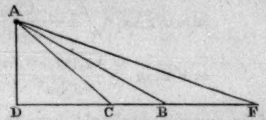
Now if the changing magnetization does work it must do it against a force, and this force must be of the character which of itself would produce magnetization, *i. e.* magnetic force. Just as when a body moving in a medium does work in the medium, it does so by calling into being, or inducing, a force, viz. friction; force being that sort of magnitude which acting upon a body produces motion.

By analogy alone, therefore, we may infer that when changing magnetization is a continuous source of absorbed work, the changing magnetization induces what would itself produce magnetization, that is an induced stress acting in opposition to the direction of the change in the magnetization.

This may perhaps be allowed; but it may be urged that there is plenty of magnetic stress already, impressed by the currents; why should not the changing magnetization work on this? The answer to this objection is that if there be no other stress but that impressed from outside, then the phase of the magnetization will be in the same phase, and *therefore the increase of magnetization* will be in quadrature with the stress, and hence no work will be continuously absorbed. For though through some phases work may be done in such a case, this is always recoverable and recovered in a complete period; a proposition which I have stated and proved in my papers upon Alternating Currents in 1885.

I apprehend, therefore, that besides the stresses AB BC we have another induced stress in quadrature with the magnetization, because called into being by its increase, and therefore in the same phase as FB or BC.

Let BC, therefore, be produced until it meets in D the line AD drawn at right angles to BC.



Then AD will be the effective magnetic stress, *i. e.* that magnetic stress which, maintained with a *steady* current, will produce the actual magnetization; hence, if M is the maximum magnetization,

$$\frac{4\pi AD}{\rho} = M,$$

where ρ is called the magnetic resistance. Thus

$$AD = \frac{M\rho}{4\pi}.$$

If 2T is the period of alternation, $\frac{\pi M}{T}$ is the maximum rate of increase of M. If

$$N\frac{\pi M}{T} = DC,$$

N may be called the coefficient of magnetic self-induction.

Under the exigencies of a certain temporary nomenclature CD might be called the Foucault-Ampere turns existing in the core itself.

Substituting for M in terms of AD,

$$DC = \frac{N\pi}{T}\frac{4\pi AD}{\rho},$$

or

$$\frac{DC}{AD} = \tan\phi = \left\{\frac{4\pi^2}{T\rho}\right\}N.$$

I have explained above how $\tan\phi$ may be obtained from the dynamometer observations. We may therefore employ this formula for the determination of the value and constancy of N, if we can rely upon the values of T obtained by observation at the time, and of ρ known otherwise.

The constancy of ρ obtains so long as magnetization can keep pace with magnetic stress. These conditions are fairly well understood, and AD must not exceed the stress representing the limiting one. Thus it will be well to know AD,

$$\begin{aligned} \mathrm{AD} = \mathrm{AB} \sin\theta &= m\mathrm{I}_1 \sqrt{1 - \cos^2\theta}, \\ &= m\sqrt{2\mathrm{A}\alpha_1}\sqrt{1 - \frac{\mathrm{C}^2\alpha_3^2}{\mathrm{A}\alpha_1\mathrm{B}\alpha_2}}, \\ &= \sqrt{2}\,.\,m\,.\,\frac{\sqrt{\mathrm{A}\alpha_1\mathrm{B}\alpha_2 - \mathrm{C}^2\alpha_3^2}}{\sqrt{\mathrm{B}\alpha^2}}. \end{aligned}$$

I think, therefore, experiment should move in the following direction. The constancy or the reverse of N under varying speeds should be first determined by experiments with currents so small or coils so few that the magnetic resistance ρ may be safely assumed constant. For this purpose it would be necessary to employ some speed-indicator.

Professor Forbes, F.R.S., in his paper upon transformers, already quoted, says that the existence of hysteresis would cause a departure from the harmonic character of the motion, but that the effects are small and negligible.

If N, however great, remains constant, the harmonic character of the variation is maintained. But if experience showed that induced stress varies as the square or higher power of the rate of change of magnetization, then indeed a serious modification would take place, and this would be likely if the neighbourhood of saturation were reached.

In the foregoing investigation I have represented *magnetic stress* as of the order *Current*, and the formulæ I have introduced hitherto will bear this convenient form of expression; but strictly speaking it is of the order *Field*, and when we wish to settle the dimensions of N we must take this into account. The M of the work is really of the order Moment or $[l^{\frac{5}{2}}t^{-1}m^{\frac{1}{2}}]$. Now the rate of increase of Moment multiplied into N is equal to Field or $[l^{-\frac{1}{2}}t^{-1}m^{\frac{1}{2}}]$;

$$\therefore\ \mathrm{N}\frac{[l^{\frac{5}{2}}t^{-1}m^{\frac{1}{2}}]}{t} = [l^{-\frac{1}{2}}t^{-1}m^{\frac{1}{2}}]$$

$$\therefore\ \mathrm{N} = [l^{-3}t].$$

The actual experiments, which I now bring to the notice of the Society, were carried out at the works of Messrs. Nalder, Brothers & Co., of Westminster, and I am greatly indebted to these gentlemen themselves, and to Mr. Crawley and Mr. Mott for their assistance in making them, for they freely placed their steam-power, their electrical power, and their intellectual power at my disposal.

These experiments had no other object than to test the question of the existence of magnetic lag by dynamometers, and to measure the angle of lag.

The machine employed and the transformer were of the Kapp forms. The numbers of turns of wire in the two coils had been ascertained for me by Mr. Crawley. They were 100 and 12 respectively.

No. of Experiment.	$A\alpha_1 = \frac{I_1^2}{2}$.	$B\alpha_2 = \frac{I_2^2}{2}$.	$C\alpha_3$.	$\frac{m}{n} C\alpha_3$.	θ.	ϕ.	$\sqrt{A\alpha_1} \sin\theta$.
1.	33·29	52·65	10·37	86·46	75° 40′	5° 43′	5·590
2.	34·43	59·74	11·50	95·83	75 19	5 38	5·676
3.	37·09	92·14	17·34	144·50	72 45	6 25	5·816
4.	70·38	86·06	17·43	145·22	77 4	5 21	8·176
5.	81·17	81·00	17·21	143·42	77 45	5 24	8·804
6.	84·97	29·03	7·83	65·26	80 56	5 4	9·103
7.	8·21	101·59	14·31	119·26	60 17	4 50	2·488

No. of Experiment.	r_1.	r_2.	$\frac{I_1}{2} r_1$.	$\frac{I_2^2}{2} r_2$.	Power of internal magnetic work ≡ H.	$\frac{H}{A\alpha_1 \sin^2\theta}$.
1.	2·01	2·51	66·91	132·15	84·96	2·72
2.	„	2·36	69·20	140·99	85·17	2·64
3.	„	1·96	74·55	180·59	102·63	3·03
4.	„	...	141·46			
5.	„	2·87	163·15	232·47	179·15	2·31
6.	„	4·77	170·79	138·47	172·82	2·09
7.	„	0·72	16·50	73·14	12·73	2·06

Considering the considerable differences in the relations of the currents, the constancy of the angle of lag appears to me to point to a simple law connecting it with the magnetization.

There was no very accurate speed-indicator employed, and the speed was approximately the same in the first six experiments. In the seventh experiment the speed was purposely much diminished, by about one third of that in the former cases, and in this experiment alone does the lag appear at less than 5°.

XLII. *Note on the Governing of Electromotors.*

By W. E. AYRTON *and* JOHN PERRY*.

IN 1882 a method was brought forward by us for governing electromotors, so that their speed should not be varied by an alteration in the load. The method, which was worked out theoretically and experimentally, was based on the combination of a motor, which converted electric energy into mechanical energy, with a brake dynamo, which reconverted the surplus mechanical energy into electric energy. After passing through various stages the arrangement finally resolved itself into an electromotor having the field-magnet wound with a shunt- and a series-coil, the coils being so connected with the rest of the circuit that the currents passed round them in opposite directions. Hence the magnetic excitation of the field-magnet was produced by the difference in the number of ampere-turns of the two circuits passing round it.

This method of governing motors, which has since 1882 been reinvented in France by M. Deprez, in America by Mr. Sprague, and which has formed the subject of an exhaustive series of experiments carried out in Germany by Dr. Fröhlich, answers well and produces a very fair constancy of speed for very considerable variations of the load. But it has a very decided defect, arising from the fact that since the series- and shunt-coils on the field-magnet oppose one another's action, the magnetization cannot be produced as economically as if there were no such differential action. Mainly on this account we have not developed our method of governing as energetically as we might otherwise have been led to do.

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* Read May 26, 1888.

In our original investigation of the subject we considered the conditions for governing motors either when a constant PD. (potential-difference) or when a constant current was supplied, and in both cases the practical solution we arrived at was a differential winding of the field-magnet. But we have since noticed that, in the case of the supply conditions being constant current, our original equations could be differently interpreted, and that the demagnetizing series could be dispensed with. Consequently a motor for constant current can be made to go at a constant speed with a varying load, and in addition to have the high efficiency of a well-made dynamo.

We shall for the general consideration of the problem use the simplest equations for a motor, disregarding saturation, as we did in our original paper, since, although the results so obtained for critical speed, &c., cannot be used directly without correction in actual practice, these equations are sufficiently accurate to show whether or not a certain combination is a possible or an impossible one for obtaining a certain required result.

If E be the back E.M.F. in the armature of a motor at a speed of n revolutions, if p be a term depending on the permanent magnetism in the iron of the field-magnet, S the current passing round the series-coil, and Z the current round the shunt-coil, we have

$$\mathrm{E}=n(p\pm q\,\mathrm{S}\pm t\mathrm{Z}). \quad . \quad . \quad . \quad . \quad . \quad (1)$$

The signs + or − in the two terms being used as the series- and the shunt-currents respectively help or oppose the permanent magnetism.

If s and z be the resistances of the series- and shunt-coils respectively, a the resistance of the armature, and A the current round the armature, we have in the case of what has been called a short shunt (fig. 1),

$$\mathrm{E}=\mathrm{Z}z-\mathrm{A}a, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and in the case of what has been called a long shunt (fig. 2),

$$\mathrm{E}=\mathrm{Z}z-\mathrm{A}(a+s); \quad . \quad . \quad . \quad . \quad . \quad (3)$$

and in both cases, if C be the constant current supplied to the motor,

$$C=A+Z. \qquad (4)$$

Fig. 1.

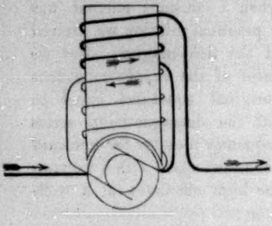
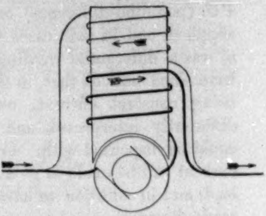


Fig. 2.



Now, remembering that in the case of a short shunt (fig. 1), C is equal to S, equations (1), (2), and (4) lead to

$$n\,\{\,p\pm q\,C\pm t\,(C-A)\,\}=(C-A)\,z-Aa; \quad . \quad . \qquad (5)$$

and in the case of a long shunt (fig. 2), remembering that C is equal to A, equations (1), (3), and (4) lead to

$$n\{\,p\pm q\,A\pm t\,(C-A)\}=(C-A)z-A\,(a+s). \quad . \qquad (6)$$

Equations 5 and 6 have then to be satisfied for all values of A for constant values of n and C; hence in the case of a short shunt,

$$\pm n\,(-tA)=-A\,(z+a), \qquad (7)$$

and in the case of a long shunt

$$\pm n(q-t)A=-A\,(z+a+s). \qquad (8)$$

But t, q, z, a, and s are all positive quantities, and since t is always larger than q, that is the number of turns in the shunt-coil is always larger than the number of turns in the series-coil, it follows that to satisfy either equation (7) for the short shunt or equation (8) for the long shunt, the positive value of t in the original equation (1) can in either case alone be employed. In other words, whatever be the arrangement of the series-coil, the shunt-coil must be a *magnetizing* and not a *demagnetizing* one.

This result we pointed out in our original paper on "Electromotors and their Government," in vol. xii. of the 'Journal of the Society of Telegraph Engineers and Electricians,' since on page 314 we stated that the solution for constant current was a shunt-motor with a series demagnetizing-coil; and it would be unnecessary to refer to this point again were it not that in one of the standard English treatises in which our method of governing motors is described in detail, the only solution for constant current that we are credited with is the *impossible* one of a series-motor with shunt demagnetizing-coil, the author not having observed that such a combination, whether used as a short or as a long shunt, could only make a motor run at a uniform speed when the speed was negative. Now, what does a negative value of *n* mean? It means that the armature must run the other way, or, in other words, that what was supposed to be a series-motor must in reality be the armature combined with a series demagnetizing-coil, that is must act as a series brake-dynamo, and the supposed shunt demagnetizing-coil must, in conjunction with the armature, act as a shunt-motor; or, in other words, the solution is as we originally stated it—a shunt-motor with series demagnetizing-coil.

In another important English work our method is given correctly, but it is stated that the method would not work, because when a greater load was put on the motor the armature would go more slowly, the back E.M.F. would therefore be diminished, and less current would pass round the shunt-coil of the field-magnet. Hence the field-magnet would be weakened, which would cause the motor to go more slowly, and so on until it stopped and possibly reversed. This objection is a curious one, because exactly the same apparent difficulty might have been stated by the author of this treatise when he was dealing with our method of governing motors to run at a constant P.D. a few pages earlier. Here, however, he sees the explanation of the difficulty, and he points out quite correctly that, although it might at first sight appear anomalous to say that, in order to keep a motor running at a fixed speed, it is necessary to weaken the field of the motor when the load on it is increased, the explanation is found in the fact that the power depends not merely on the strength of the field but on the current passing through the armature, and that this latter is much increased when the field is weakened. Hence, on the whole, it is quite right to arrange matters so that the field is weakened when the load is increased. We say it is curious then, that while the author has explained the apparent difficulty so clearly when he was dealing with our method for governing motors when the supply condition was a constant P.D., he does not see that exactly the same explanation clears away the difficulty which he has himself raised regarding our method for governing motors to run when supplied with constant current.

What, however, we particularly wish to point out in this short note is that when the condition of supply is constant current it is not necessary to have a series demagnetizing-coil at all, and consequently the motor can be made to run at constant speed independently of the load, and in addition to work with high efficiency.

We will first see whether such a solution is possible when the condition of supply is constant P.D. Let the motor be a simple shunt-motor; then, if as before E be the back E.M.F., n the speed, p a term depending on the permanent magnetism, and Z the current round the shunt,

$$E = n\,(p + t\,Z); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (9)$$

also

$$E = Zz - Aa,$$

if z be the resistance of the shunt-coil, A the current in the armature, and a the resistance of the armature; also

$$V = \frac{Z}{z},$$

where V is the constant P.D. maintained at the terminal, of the motor;

$$\therefore \quad n\left(p + t\frac{V}{z}\right) = V - Aa. \quad . \quad . \quad . \quad . \qquad (10)$$

If n is to be constant as well as V, the left-hand side of the equation is constant, whereas the right-hand side varies with A the current through the armature, which must alter as the power given out by the motor alters. One solution for making this equation always true independent of the value of A is the well-known one of making a very small. When, however, a is not very small, and we have shown in detail elsewhere why we consider it a mistake to make the resistance of the armature as small in a motor as in a dynamo, equation (10) cannot be fulfilled for constant values of n and V, and a variable value of A; and in order that the equation may be made true a negative term proportional to A must be introduced on the left-hand side. The simplest way of doing this is to add a series demagnetizing-coil. Then (9) becomes

$$E = n\,(p - q\,A + tZ),$$

and (10)

$$n\left(p - q\,A + t\,\frac{V}{z}\right) = V - A\,(a + s),$$

an equation which will be always satisfied for any value of A if

$$n = \frac{a+s}{q}.$$

But when the supply is a constant current C supplied to the motor, the equation for a simple shunt-motor is

$$E = n\,(p + tZ),$$

$$E = Zz - Aa,$$

$$C = A + Z\,;$$

and, therefore,

$$n\,\{p + t\,(C - A)\} = (C - A)z - Aa. \quad . \quad . \quad . \quad (11)$$

A now appears with a negative sign on both sides of the equation (11), and this equation will always be satisfied for any value of A if

$$n = \frac{z+a}{t}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

without there being any necessity for using a series demagnetizing-coil. At first sight, then, it appears as if an entirely new solution for governing a motor supplied with a constant current had been arrivéd at, consisting simply of using a shunt-motor. But on further examination it is seen that if we substitute in (11) the value given in (12) for n, we obtain for C the current for which the motor governs:

$$C = \frac{np}{z - nt}$$

$$= -\frac{np}{a}.$$

But as this is negative it is necessary, in order that the shunt-motor may govern, that the terminals of the motor should be reversed. This will reverse the current round the field-magnet but, by hypothesis, not the residual magnetism, otherwise p would also become negative. The core of the electromagnet must, therefore, be of steel, so strongly magnetized that the shunt-current does not reverse the polarity, and the permanent magnet and rotating armature together form a brake-dynamo while the shunt-coil and the armature will form the motor. But this is simply one of the solutions (a theoretical one of course and not a practical one) given by us on page 310 of our original paper and illustrated in figure 10 on that page.

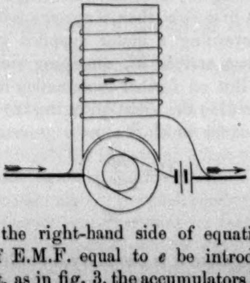
The fact, however, still remains that in the case of the supply condition being constant current, equation (11) can be satisfied without the employment of a series demagnetizing-coil; and all that we have to do to in order to make the solution a practical one, that is to enable an ordinary field-magnet with soft iron core to be employed, is to alter the equation so that when the speed equation (12) is satisfied (11) leads to a positive value of the current. This result may be obtained by adding to the right-hand side of equation (11) a positive constant term e numerically larger than np, for in that case, if

$$n=\frac{z+a}{t}$$

$$C=\frac{e-np}{a}$$

for all values of A. Now such a positive term e will be introduced on the right-hand side of equation e, if some accumulators of E.M.F. equal to e be introduced into the armature circuit, as in fig. 3, the accumulators being joined up so as to help the main current C. In that case the resistance

Fig. 3.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a of the armature must be increased by a', the resistance of the accumulators; and the two equations we are finally led to for the speed at which the motor governs, and the constant current for which it governs, are

$$n=\frac{z+a+a'}{t}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

$$C=\frac{e-np}{a+a'}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

This solution is a practical one, and consists of a simple shunt-motor, with some accumulators in the armature circuit. Further, it is one not requiring many accumulators, for if np, the back E.M.F. of the motor due to the residual magnetism alone be small, as it will be if the field-magnet iron core be soft, and further, if $a+a'$ be also not large, e need not be large in order that the value of C, the current for which the motor governs, may be as large as we like. The essence of this new method for making a motor supplied with a constant current revolve at a constant speed independently of the load does not consist in combining a shunt-motor with a series brake-dynamo, which was the essence of our original method, but in combining a shunt-motor with a store of electric energy in the armature-circuit, which store of energy, as the equations show, is automatically dealt out to help the motor exactly in proportion as the demand for power is required to keep the speed constant when the load on the motor is varied. And this store can be easily maintained by reversing the connexions of the accumulators when they are disconnected from the motor, and the motor is at rest, and leaving the accumulators without supervision to be charged by the constant current that is continuously supplied to the station.

While on the subject of electromotors it may be well to point out a fact which we thought was well known, viz. that when motors are being tested one against another for efficiency and for power developed per pound weight, it is most important that the motors should each be supplied with the P.D. they are intended to work at and should be run at their normal speed. But we have been much astonished to find in the 'Technology Quarterly' for September last, published by the Massachusetts Institute of Technology, a paper by Mr. H. Clifford on "The Efficiency of Small Electro-Motors," in which the results of experiments on the relative power and efficiency of a number of motors are given without any attention having been paid to the particular P.D. and speed at which each motor was wound to work at. Mr. Clifford's results are arranged in tables which would be of considerable practical value were it not that the essential condition for making such tests has been absolutely disregarded. Hence Mr. Clifford's tables are not only valueless but are most misleading. We are able to speak definitely on the subject, because the results of the tests made on a motor of ours, which are quoted, are made with the motor running at less than half its normal speed, and supplied with less than one quarter of its normal power. As well might incandescent lamps intended to be run with very different P.Ds. be compared when run with totally wrong P.Ds., and a 100-volt lamp condemned as useless because when supplied with only 50 volts it emitted but little light.

XLIII. *On the Formulæ of* Bernoulli *and of* Haecker *for the Lifting-power of Magnets.* *By* Professor SILVANUS P. THOMPSON[*].

FORMULÆ for the lifting-power of magnets have been given by D. Bernoulli and by Haecker, and more recently by Van der Willigen.

Bernoulli's empirical rule[†] is that the lifting-power varies as the cube root of the square of the magnet's weight, or

$$P \propto \sqrt[3]{W^2},$$

where P is the lifting power or pull exerted by the magnet in contact with its keeper, and W the mass of the magnet.

Haecker[‡] gave precision to the rule by introducing a coefficient, the numerical value of which varies with the qualities of steel employed. Writing

$$P = a\sqrt[3]{W^2},$$

he found that (when the unit of mass taken was the German pound) *a* had values between 8 and 10 for bar-magnets, and double these values for horseshoe-magnets. That is to say, according to Haecker a steel horseshoe-magnet weighing 1 pound could lift between 16 and 20 pounds, according to the quality of the steel and the intensity of its magnetization. The formula was found to be reasonably valid for magnets between $\frac{1}{120}$ pound up to 40 pounds in weight.

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[*] Read May 26, 1888.

[†] *Acta Helvetica*, iii. p. 233 (1758).

[‡] Pogg. *Ann.* lvii. p. 321 (1842).

Van der Willigen *, repeating the investigation partly with Haecker's own magnets and partly with more recent magnets made by Von Wetteren, by Elias, and by Logeman, found a somewhat different empirical rule. According to him the permanent lifting-power should be written

$$P = bK\sqrt{A}\sqrt[4]{\frac{L}{\sqrt{A}}} \times \frac{L}{l};$$

where b is a constant depending on the quality of the steel and its degree of magnetization, K the perimeter of the polar surface, A the area of polar surface, L the length between the actual poles, and l the mean between interior and exterior lengths of the bar. For similar solids $K\sqrt{A}$ is simply proportional to the surface A; so that this formula if applied to similar magnets merely means that the lifting-power varies as the polar surface, multiplied by a correcting factor which takes into account the proximity of the two poles. Van der Willigen found the coefficient b, which is virtually the same as Haecker's a, to vary, for good horseshoe-magnets, from 19·5 to 22·5.

If we consider the meaning of Bernoulli's and Haecker's formulæ, as applied to magnets of similar form, we shall see that the cube root of the square of the mass of the magnet is a quantity simply proportional to its polar surface, for the linear dimensions are proportional to the cube root of the mass, and the surface to the square of the linear dimensions. Hence, so far as similar forms of magnet are concerned, these rules mean nothing more or less than that the lifting-power is proportional to the polar surface. This we know from Joule's and many subsequent researches to be approximately true for magnets carried to equal intensity of magnetization.

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* *Sur le magnétisme des aimants artificiels*, par V. S. M. Van der Willigen (Haarlem, 1878).

Viewing the matter by the light of more recent researches on the induction of magnetism in closed circuits of iron or steel, we come to the same conclusion; for assuming that the magnetic induction B has been carried to an equal degree in the metal, the tension at any point in the circuit (in dynes per square centimetre) is*

$$T = \frac{B^2}{8\pi},$$

and the element of the pull over area dA is

$$dP = TdA = \frac{1}{8\pi} B^2 dA;$$

whence

$$P = \frac{1}{8\pi} \int B^2 dA,$$

which can be determined if the law of the distribution of the magnetic induction through the cross-section is known. Assuming this to be a simple uniform distribution (it is generally not quite so at the joint between the polar surfaces and armature of a magnet), this gives the pull (in dynes) as

$$P = \frac{1}{8\pi} B^2 A.$$

This formula † affords a very convenient method of reckoning B from measurements made upon the pull exerted at a given polar surface; the formulæ becoming

$$B = 5000 \sqrt{\frac{P \text{ kilos.}}{A \text{ sq. cm.}}};$$

or

$$B = 1317 \sqrt{\frac{P \text{ lbs.}}{A \text{ sq. in.}}}.$$

The a of Haecker's formula may, therefore, be taken as simply proportional to the square of the magnetic induction through the contact-surface, or

$$a = \frac{1}{8\pi} B^2 . d^{\frac{2}{3}} . c,$$

where d is the density of the steel, and c the ratio of the polar surface to the surface of one face of a cube of equal volume to that of the magnet.

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* Maxwell's 'Electricity and Magnetism,' Art. 643.

† In Mr. Shelford Bidwell's paper, Proc. Roy. Soc. 1886, he uses a formula equivalent to $\frac{1}{8\pi}(B^2 - H^2)A$, but without giving any reason for deducting H^2 from B^2.

Haecker found *a* for horseshoe-magnets twice as great as for bar-magnets. Van der Willigen found it from three to four times as great for horseshoes. Taking Haecker's figure, this shows that the long return-path through air of the tubes of magnetic induction offered so great a resistance that the steel magnet could only produce across the polar surface in this case an induction $\frac{1}{\sqrt{2}}$ times as great as when the closed horseshoe circuit was used.

Consideration of the rational formula will show that the greater lifting-power in proportion to their own weight possessed by small magnets, does not require for its explanation the sometimes alleged fact that small pieces of steel can be more highly magnetized than large pieces of steel. For, assuming equal intensity of magnetic induction, B, it is seen that the lifting-power is proportional to surface and not to weight; hence it must necessarily be greater relatively to weight in small magnets.

The net result of this paper is that Haecker's (and Bernoulli's) formula is merely another way of saying that the lifting-power of magnets in which the intensity of the magnetic induction has been carried up to an equal degree, is proportional to the polar surface. And Haecker's coefficient is proportional to B^2 through that surface.

XLIV. *The Function of Osmotic Pressure in the Analogy between Solutions and Gases.* *By* Professor J. van't Hoff[*].

During an investigation which required some knowledge of the laws regulating chemical equilibrium in solutions, the conclusion has gradually been evolved that a deep analogy —indeed almost an identity—exists between dilute solutions exerting osmotic pressure on the one hand, and gases under ordinary atmospheric pressure on the other. The following pages contain an attempt to explain this analogy; and the physical properties of such systems will form the first subject of discussion.

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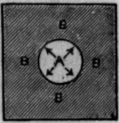
[*] Read June 9, 1888; translated by Prof. W. Ramsay, F.R.S.

I. *Osmotic Pressure—the nature of the Analogy due to this conception.*

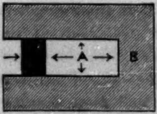
In order clearly to realize the quantity referred to as osmotic pressure, imagine a vessel, A (fig. 1), completely full of an aqueous solution of sugar, placed in water, B. If it be conceived that the solid walls of this vessel are permeable to water, but impermeable to the dissolved sugar, then, owing to the attraction of the solution for water, water will enter the vessel A up to a certain limit, thereby increasing the pressure on the walls of the vessel. Equilibrium then ensues, owing to the pressure resisting further entry of water. This pressure we have termed *osmotic pressure*.

Fig. 1.



It is evident that this state of equilibrium might have been attained in A without entry of water if the vessel had been constructed with a piston, compressing the solution with a pressure equal to the osmotic pressure (fig. 2). It follows moreover that, by increasing or diminishing the pressure on the piston, the state of concentration of the liquid can also be altered, owing to the passage of water through the walls of the vessel in an outward or inward direction.

Fig. 2.



Such osmotic pressure has been experimentally investigated by Pfeffer (*Osmotische Untersuchungen*, Leipsig, 1887). The walls of the cell consisted of unglazed porcelain, rendered impermeable to sugar though not to water, by filling it with a solution of potassium ferrocyanide and placing it in a solution of copper sulphate. Owing to diffusion, the ferrocyanide and the copper-salt come in contact after some time, and produce a membrane of copper ferrocyanide having the required properties. Such a vessel is then filled with a one-per-cent. solution of sugar; it is then closed with a cork provided with a manometer, and sunk in water; the osmotic pressure gradually rises, owing to entry of water, and the pressure due to the entry of water is read off when it becomes constant. As an example of the results obtained, it may be mentioned that a one-per-cent. solution of sugar (which, owing to its considerable mass, was not appreciably diluted on entry of water) exerted at 6°·8 a pressure of 50·5 millim. of mercury—about one fifteenth of an atmosphere.

The porous membrane, such as that described, will be termed in the following pages a "semipermeable membrane;" and the conception will be made use of even where experimental verification is lacking. The behaviour of solutions may thus be studied in a manner strikingly analogous to that employed in the study of gases, inasmuch as what is known as "osmotic pressure" corresponds to pressure, or, as it is commonly but incorrectly termed, "tension" of a gas. It is right to mention that this is no fanciful analogy, but a fundamental one; the mechanism which, according to our present views, controls the pressure of gases and the osmotic pressure of liquids is substantially the same. In the former case pressure is due to the impacts of gaseous molecules on the walls of the containing vessel, and in the latter to the impacts of the molecules of the dissolved substance on the semipermeable membrane, since the impacts of the molecules of the solvent, being equal and opposed on each side of the vessel, may be neglected.

The great practical advantage arising from this method of regarding the behaviour of solutions, which leads at once to quantitative conclusions, consists in the fact that the application of the second law of thermodynamics to liquids is rendered exceedingly easy; for reversible processes, of which this law treats, can now be very simply conceived. It has already been mentioned that a piston and cylinder with semipermeable walls, placed in the solvent, permits of alteration of concentration of the solution contained therein, by alteration of the pressure on the piston, in exactly the same way as a gas can be rarefied or compressed; except that in the former case it is the solvent which escapes through the semipermeable walls on increase of pressure. Processes of this kind can always be made reversible, if care be taken that the pressure on the piston is made equal to the opposed pressure, that is, in the case of solutions, the osmotic pressure.

We shall make use of this practical advantage in the following pages, particularly in investigating the laws of "ideal solutions;" that is to say, solutions so dilute as to be comparable with "ideal" or "perfect" gases, in which the action on one another of the dissolved molecules, as well as their actual volume compared with that of the space they inhabit, is so small as to be negligible.

II. Boyle's *Law for Dilute Solutions.*

The analogy between dilute solutions and gases acquires at once a quantitative form, if it be noted that in both cases alteration of concentration exercises a similar influence on pressure, and is in both cases proportional to the pressure.

This proportionality, which for gases goes by the name of Boyle's law, may be proved experimentally for liquids, as well as deduced theoretically.

Experimental Proof (Determination of osmotic pressure for solutions of various concentrations).—Let us first adduce Pfeffer's determinations (*Osmotische Untersuchüngen*, p. 71) of the osmotic pressure (P) in sugar-solutions at the same temperature (13°·2 to 16°·1), and with varying concentrations (C):—

C.	P.	$\frac{P}{C}$.
1 per cent.	535 millim.	535
2 „	1016 „	508
2·74 „	1518 „	554
4 „	2082 „	521
6 „	3075 „	513

The approximately constant quotients $\frac{P}{C}$ point conclusively to this proportionality between pressure and concentration.

Comparison of Osmotic Pressures by Physiological Methods.—Observations of de Vries ("Eine Methode zur Analyse der Turgorkraft," Pringsheim's *Jahrb.* xiv.) show that equal changes of concentration of solutions of sugar, and of potassium sulphate and nitrate, exercise equal influence on the osmotic pressure. This osmotic pressure was compared, by physiological methods, with that of the contents of a plant-cell; the protoplasmal envelope contracts when it is immersed in solutions possessing great attraction for water. By a systematic comparison of the three bodies mentioned, using the same cells, three *isotonic liquids* (*i. e.* liquids exhibiting the same osmotic pressure) were obtained. Cells of a different plant were then made use of, and so four *isotonic* series were constructed which showed a similar proportion in their concentrations; this is exhibited in the following table, where the concentrations are expressed in gram-molecules per litre:—

Series.	KNO_3.	$C_{12}H_{22}O_{11}$.	K_2SO_4.	$KNO_3=1$.	$C_{12}H_{22}O_{11}$.	K_2SO_4.
I. .	0·12	—	0·09	1	—	0·75
II. .	0·13	0·2	0·1	1	1·54	0·77
III. .	0·195	0·3	0·15	1	1·54	0·77
IV. .	0·26	0·4	—	1	1·54	—

Theoretical Proof.—These observations render highly probable the existence of proportionality between osmotic pressure and concentration, and the theorem may be completed by a theoretical proof which is, indeed, almost self-evident. Regarding osmotic pressure as due to a kinetic cause (*i. e.* as produced by impacts of the dissolved molecules), there must exist a proportionality between the number of impacts in unit time and the number of molecules in unit volume. The proof is therefore exactly the same as that for Boyle's law. If, on the other hand, osmotic pressure be regarded as the outcome of an attraction for water-molecules, its value is evidently proportional to the number of attracting molecules in unit volume, provided (and this is taken for granted in sufficiently dilute solutions) the dissolved molecules exercise no attraction on each other, and each one exerts its own special attractive action, uninfluenced by its neighbours.

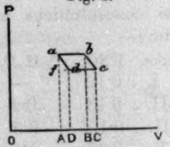
III. Gay-Lussac's *Law for Dilute Solutions.*

While the proportionality between concentration and osmotic pressure is self-evident, so long as temperature remains constant, the proportionality between osmotic pressure and absolute temperature, the concentration being maintained constant, is not so manifest. Yet proof can be furnished from thermodynamical considerations; and experimental data exist which are highly favourable to the results predicted on thermodynamical grounds.

Theoretical Proof.—It has been already mentioned that, by means of a piston and a cylinder with semipermeable walls, reversible processes can be conceived to occur. If such processes are expressed in the way common as regards gases, volume and pressures are indicated on the lines O V and O P (fig. 3); but pressure in this case, as before, must be taken as osmotic pressure. The initial volume (V cub. metres) is represented by OA; the initial pressure on the piston of area 1 square metre (P kilogr.) by Aa; and the absolute temperature by T; the solution is then conceived to undergo a minute increase of volume, dV cubic metres (=AB), by moving the piston through dV metres, while the temperature of the solution is maintained constant by introduction of the requisite amount of heat. This amount of heat can be at once determined, inasmuch as it is equivalent to the external work performed, PdV, by the motion of the piston. Internal work is absent, for the dilution is, by hypothesis, so great that the dissolved molecules exercise no attraction on each other This isothermal change ab is succeeded by the isentropic or adiabatic change bc, during which heat is neither absorbed nor evolved; the temperature sinks by dT°; and the original condition is then brought about by a second isothermal and a second adiabatic change, cd and da, respectively. The second law of thermodynamics requires that the fraction, $\frac{dT}{T}PdV$ of the initially imparted heat charge PdV shall have been converted into work; this must be equivalent to the area $abcd$; and hence the equation follows: $\frac{dT}{T}PdV = a\,b\,c\,d = af \,.\, AB = af\,dV$; and hence $P\frac{dT}{T} = af$. But af represents the change of osmotic pressure, volume being kept constant, due to the change of temperature dT; *i. e.* $\left(\frac{dP}{dT}\right)_V dT$; hence

$$\left(\frac{dP}{dT}\right)_V = \frac{P}{T}.$$

Fig. 3.



On integration this equation gives, on the assumption of constant volume,

$$\frac{P}{T} = \text{constant};$$

that is, the osmotic pressure is proportional to the absolute temperature, provided concentration (which is here equivalent to the volume of a gas) remain constant; and this is entirely in accordance with Gay-Lussac's law connecting the pressure and temperature of gases.

Experimental Proof (*Determination of the osmotic pressure at different temperatures*).—Let us compare this theoretical conclusion with the experimental data furnished by Pfeffer (pp. 114–115). He found that the osmotic pressure increases with rise of temperature; it will be seen that, although his results do not furnish a conclusive proof of the correctness of the theorem, yet there is a most striking correspondence between experiment and theory. If we calculate from one of two experiments at different temperatures the osmotic pressure to be expected in the other, by help of Gay-Lussac's law, and compare it with the experimental result, we have the following series :—

1. Solution of cane-sugar.

    Pressure at 32°, found . . . 544 millim.
    Pressure at 14°·15, calculated . 512 „
    „ „ found . . 510 „

2. Solution of cane-sugar.

    Pressure at 36°, found . . . 567 „
    Pressure at 15°·5, calculated . 529 „
    „ „ found . . 520·5 „

3. Solution of sodium tartrate.

    Pressure at 36°·6, found . . 1564 „
    Pressure at 13°·3, calculated . 1443 „
    „ „ found . . 1431·6 „

4. Solution of sodium tartrate.

    Pressure at 37°·3, found . . 983 „
    Pressure at 13°·3, calculated . 907 „
    „ „ found . . 908 „

Comparison of the Osmotic Pressure by Physiological Methods.—In the same manner that support has been lent to the application of Boyle's law to solutions (viz. that different substances in *isotonic* solutions retain their equality of osmotic pressure so long as their respective concentrations are reduced to the same fraction), so the application of Gay-Lussac's law receives support by the fact that this isotonic state is maintained during equal alterations of temperature. It has been proved by physiological methods by Donders and Hamburger (*Onderzoekingen gedaan in het physiologisch. Laboratorium der Utrechtsche Hoogeschool* [3] ix. p. 26), making use of blood-corpuscles, that solutions of potassium nitrate, sodium chloride, and sugar, which at 0° are isotonic with the contents of these cells, and hence are isotonic with each other, exhibit the same isotonic state at 34°; this is seen in the annexed table:—

	Temperature 0°.	Temperature 34°.
KNO . . .	1·052 to 1·03 p. c.	1·052 to 1·03 p. c.
NaCl . . .	0·62 to 0·609 p. c.	0·62 to 0·609 p. c.
$C_{12}H_{22}O_{11}$ . .	5·48 to 5·38 p. c.	5·48 to 5·38 p. c.

Experimental Proof of Boyle's *and* Gay-Lussac's *Laws for Solutions. Experiments by* Soret (*Archives des Sciences phys. et nat.* [3] ii. p. 48; *Ann. Chim. Phys.* [5] xxii. p. 293).—The phenomenon observed by Soret lends a strong support to the analogy between dilute solutions and gases in respect of the influence of concentration and temperature on pressure. His work shows that just as in gases the warmest part is the most rarefied, so with solutions the warmest portions are the most dilute; but that in the latter case a much longer time must be allowed for the attainment of equilibrium. The experimental apparatus consisted of a vertical tube, the upper portion of which was heated while the lower portion was kept at a low temperature.

Soret's latest experiments lend a quantitative support to our analogy. As with gases, it is to be expected that when the isotonic state is produced, the solution will exist in equilibrium; and as the osmotic pressure is proportional to concentration and to absolute temperature, the isotonic state of different portions of the solution will occur when the product of the two (absolute temperature and concentration) are equal. If we therefore calculate, on this basis, the concentration of the warmer part of the solution from data obtained with the colder, the values compare with those found as follows:—

1. *Solution of Copper Sulphate.*—The portion cooled to 20° contained 17·332 per cent. The hot portion at 80° should contain 14·3 per cent.; found, 14·03 per cent.

2. The portion cooled to 20° contained 29·867 per cent. The portion at 80° should contain 24·8 per cent.; found, 23·871 per cent.

It must be stated that previous experiments by Soret gave less favourable results; yet perhaps too much importance should not be attached to them, owing to the difficulties of experiment.

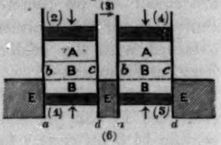
IV. Avogadro's *Law applied to Dilute Solutions.*

We have considered the changes produced in the osmotic pressure of solutions by alteration of temperature and concentration, and attempted to exhibit the analogy between dilute solutions and gases, in relation to these two quantities It now remains to compare directly the two analogous quantities, gaseous pressure and osmotic pressure, in one and the same body. It is obvious that this analogy should hold with gases in solution; and in actual fact it will be shown that, if Henry's law be taken into consideration, the osmotic pressure in solution is absolutely equal to the gaseous pressure, under similar conditions of temperature and concentration.

To prove this statement, we shall picture a reversible process by aid of semipermeable diaphragms, temperature being maintained constant; and we shall again make use of the second law of thermodynamics, which in this case leads to the simple result that no work is transformed into heat, nor heat into work; and hence the sum of all work done at different stages of the process is zero.

This reversible process may be conceived by means of two similar cylinders and pistons, like those already described. One contains a gas (A), say oxygen, and in contact with it a saturated aqueous solution of oxygen (B) (fig. 4). The wall *b c*

Fig. 4.



allows only oxygen to pass, but no water; the wall ab, on the other hand, water, but not oxygen; and it is in contact with water, E. A reversible process may be carried out by such an arrangement as follows:—By raising the two pistons (1) and (2), oxygen is evolved from its aqueous solution as gas, while the water passes through ab; this change can proceed without altering the concentration of the solution. The only difference between the two cylinders is in the state of concentration of the solutions which they contain; we may explain the action thus:—The unit of weight of the substance in question occupies in the left-hand cylinder a volume v and V, and in the right-hand cylinder, $v+dv$ and $\mathrm{V}+d\mathrm{V}$; hence, in order that Henry's law may hold,

$$v : \mathrm{V} = (v+dv) : (\mathrm{V}+d\mathrm{V})\text{; hence } v : \mathrm{V} = dv : d\mathrm{V}.$$

If, now, the pressure, or osmotic pressure, as the case may be (supposing unit volume to contain unit mass), as regards gas and solution be P and p (which will afterwards be shown to be equal), then, from Boyle's law, the pressure in gas and solution will be $\frac{\mathrm{P}}{v}$ and $\frac{p}{\mathrm{V}}$ respectively.

Now let the pistons (1) and (2) be raised so as to liberate unit weight of gas from the solution, and increase the gaseous volume v by dv, in order to equalize its concentration with that of the gas in the left-hand vessel, and by depressing the pistons (4) and (5) let us redissolve the freshly liberated gas, and then reduce the volume of the solution $\mathrm{V}+d\mathrm{V}$ by the amount $d\mathrm{V}$ in the cylinder with semipermeable walls; then the cycle is complete.

Work has been done in six separate ways; let us number them (1), (2), (3), (4), (5), and (6). Now (2) and (4) are equal in amount, but opposite in sign, since they refer to change of volume v and $v+dv$ in opposite directions under pressures which are inversely proportional to the volumes. In similar manner the sum of (1) and (5) is zero; so that the point requiring proof is that $(3)+(6)=0$. Here (3) represents work done by the gas in increasing its volume by dv, under a pressure of $\frac{P}{v}$, therefore $(3)=\frac{P}{v}dv$; while (6) represents the work done by the solution, while it decreases in volume by dV, under a pressure of $\frac{p}{V}$, therefore $(6)=-\frac{p}{V}dV$. The statement is therefore

$$\frac{P}{v}dv=\frac{p}{V}dV;$$

and as $v:V=dv:dV$, P must be equal to p, which was to be proved.

This conclusion, which will receive in the sequel ample confirmation, lends, on the one hand, support to Gay-Lussac's law in its application to liquids:—If gaseous pressure and osmotic pressure are at the same temperature equal to one another, then equal changes of temperature must affect both equally. On the other hand, this relation allows of a considerable extension of Avogadro's law, always provided that we may substitute the conception of osmotic pressure for gaseous pressure:—under equal osmotic pressures and at the same temperature, equal volumes of all solutions contain equal numbers of molecules; and, moreover, the same number of molecules which would be contained in an equal volume of a gas under the same conditions of temperature and pressure.

V. *General Expression for* Boyle's, Gay-Lussac's, *and* Avogadro's *Laws for Solutions and Gases.*

The well-known formula expressing both Boyle's and Gay-Lussac's laws for gases,

$$PV=RT,$$

are, in so far as these laws are applicable to liquids, also applicable as regards osmotic pressure; with the reservation, also made in the case of gases, that the space occupied by the molecules must be so great that the actual volume of the molecules becomes negligible.

To include Avogadro's law in the above expression, Horstmann's suggestion (*Berl. Ber.* xiv. p. 1243) may be adopted, to express the molecular weight of the substance in kilograms;

taking 2 kilos. of hydrogen, 44 kilos. of carbon dioxide, and so on. Then R in the above equation has always the same value; for, under equal conditions of temperature and pressure, these weights occupy the same volume. Calculating this value, and expressing the volume in cubic metres, and the pressure in kilograms per square metre, and choosing hydrogen gas at 0° and 760 millim. pressure as starting-point, then

$$P=10333; \quad V=\frac{2}{0{\cdot}08956}; \quad T=273; \quad R=845{\cdot}05.$$

Hence the combined expression for Boyle's, Gay-Lussac's, and Avogadro's laws becomes

$$PV=845T;$$

and this expression is applicable to solutions, substituting osmotic pressure for gaseous pressure.

We may give this equation even a simpler form, inasmuch as the number of calories equivalent to a kilogrammetre of work $(A=\frac{1}{J}=\frac{1}{423})$ stands in a very simple relation to R, viz. $AR=2$ (in reality about one thousandth less). Hence we may choose the form

$$APV=2T;$$

and this form has the great practical advantage that work, which will often be discussed in the following pages, receives a very simple expression, if calculated back to heat, measured in calories.

Let us next calculate the work, expressed in calories, when a gas or a solution, under constant pressure and temperature, expands V volumes; V volumes containing a kilogram-molecule. This is evidently 2T. It must be added that this constant pressure is maintained only when the total volume of gas or solution is very great compared with V, or in cases of evaporation, where the vapour-pressure is at its maximum.

We shall also often have to express in calories the work done during isothermal expansion of the kilogram-molecule of a substance as gas, or in solution. If pressure falls a very small fraction ΔP, corresponding to a small increase of volume ΔV, the work done will be $AP\Delta V$, or $2\Delta T$.

VI. *First Confirmation of* Avogadro's *Law in its Application to Solutions.—Direct determination of Osmotic Pressure.*

It is to be expected that Avogadro's law, deduced as a consequence of Henry's law for solutions of gases, will not be restricted to solutions of substances which usually exist in a gaseous condition. This expectation has been realized, not merely from a theoretical, but from an experimental standpoint. Pfeffer's determinations of the osmotic pressure of solutions of sugar furnish a remarkable confirmation of this extension of the law.

Pfeffer's solution consisted of 1 gram of sugar dissolved in 100 grams of water; one gram of the sugar therefore exists in about 100·6 cubic centim. of the solution. Comparing the osmotic pressure of this solution with the pressure of a gas (*e. g.* hydrogen) containing as many molecules in the volume, there are $\frac{2}{342}$ gram ($C_{12}H_{22}O_{11}=342$) in 100·6 cubic centim. Now one litre of hydrogen gas at 0° and 760 millim. pressure weighs 0·08956 gram; and the above concentration is equivalent to 0·0581 gram per litre; the pressure at 0° is 0·649 atmosphere, and at t, $0{\cdot}649(1+0{\cdot}00367\,t)$. Placing these results beside Pfeffer's, we obtain the following agreement:—

Temperature (t).	Osmotic pressure.	$0{\cdot}649(1+0{\cdot}00367\,t)$.
6·8	0·664	0·665
13·7	0·691	0·681
14·2	0·671	0·682
15·5	0·684	0·686
22·0	0·721	0·701
32·0	0·716	0·725
36·0	0·746	0·735

The directly determined osmotic pressure of a solution of sugar is thus seen to be equal to the pressure of a gas at the same temperature, containing the same number of molecules in unit volume as the sugar-solution.

Starting from cane-sugar, this relation can be calculated for other dissolved substances, such as invert sugar, malic acid, tartaric acid, citric acid, magnesium malate and citrate, all of which, from de Vries' physiological researches (*Eine Methode zur Messung der Turgorkraft*, p. 512), exhibit equal osmotic pressure when they contain an equal number of molecules in a given volume.

VII. *Second Confirmation of* Avogadro's *Law in its Application to Solutions.—Molecular lowering of Vapour-pressure.*

The relation between osmotic pressure and the pressure of a vapour in contact with liquid, which is easily developed on thermodynamical principles, yields, from Raoult's recent observations, a satisfactory proof of the analogy under consideration.

We shall begin with a general law, of which the previous matter of this thesis is quite independent:—*Isotonism* (*i. e.* equal osmotic pressure—from ἴσος and τόνος, *stretching*) *in solutions made with the same solvent, implies equality of vapour-pressure.* This statement is easily proved by a reversible cycle, keeping temperature constant. Imagine two solutions exhibiting equal vapour-pressure, and introduce a small quantity of one in the state of vapour into the other in a reversible manner, *i. e.* by means of cylinder and piston. The vapour-pressures are equal, hence this transference takes place without expenditure of work; and also, on restoring the original condition, no work is expended. But if the solvent be transferred back through a semipermeable diaphragm, separating the two solutions, then equal osmotic pressure must exist, else the transference could not take place without expenditure of energy.

If this fundamental conception be applied to dilute solutions, accepting the laws which have been explained in the preceding pages, the simple conclusion follows, that if a solvent contains equal numbers of molecules of dissolved substances, the vapour-pressure is the same. This is merely Raoult's law (*Comptes rendus*, lxxxvii. p. 167; xliv. p. 1431) of the constancy of molecular diminution of vapour-pressure, obtained by multiplying the relative diminution by the molecular weight of a one-per-cent. solution; *i. e.* with the difference between the vapour-pressures of the solvent, before and after addition of dissolved substance. The equality of molecular diminution of vapour-pressure depends on the solutions containing equal numbers of molecules, a close proportionality between the lowering of the vapour-pressure and the concentration being assumed. With ether, for example, the value fluctuated between 0·67 and 0·74 (mean 0·71) for thirteen substances dissolved in it.

But this relation can be further developed. Different solutions may be compared with each other, and a second law may be deduced, which Raoult has also discovered experimentally. The following reversible process, consisting of two operations, is carried out with a very dilute solution of P per cent., at temperature T.

1. Remove, by use of cylinder with semipermeable wall, a portion of the solvent containing a kilogram-molecule (M) of the dissolved substance: here the total quantity of solution is supposed so great that no alteration of concentration occurs, and the expenditure of work is therefore 2T.

2. This quantity, $\frac{100\,M}{P}$ kilograms, of the solvent is returned as vapour; it may be conceived as produced from the liquid by evaporation at its vapour-pressure; then expanded till its pressure is equal to the vapour-pressure of the solution; and finally liquefied in contact with the solution. The kilogram-molecule of the solvent (M′) will thus receive an expenditure of work of $2T\Delta$, where Δ represents the relative diminution of pressure; and the $\frac{100\,M}{P}$ kilograms will receive $2T\Delta\frac{100\,M}{PM'}$. Here $\frac{\Delta}{P}M$ is Raoult's molecular diminution of pressure, which we shall term K; employing this abbreviation, the expression becomes $\frac{200\,TK}{M'}$.

From the second law of thermodynamics, again, the algebraic sum of the work expended during this cycle at constant pressure must equal zero; hence the work done by the solution during the first stage must equal the work done on it during the second; and thus

$$2T = \frac{200\,TK}{M'}; \text{ or } 100\,K = M'.$$

This expression includes all Raoult's results. It expresses the fact, as stated above, that the molecular diminution of vapour-pressure is independent of the nature of the dissolved body; and it also expresses, what Raoult found experimentally, that this diminution is independent of temperature. It also contains Raoult's second conclusion, that the molecular diminution is proportional to the molecular weight of the solvent, amounting to about one hundredth of the latter. This is seen from the following table:—

Solvent.	Molecular weight. (M').	Molecular diminution of vapour-pressure. (K).
Water	18	0·185
Phosphorous chloride	137·5	1·49
Carbon disulphide	76	0·80
Carbon tetrachloride	154	1·62
Chloroform	119·5	1·30
Amylene	70	0·74
Benzene	78	0·83
Methyl iodide	142	1·49
Methyl bromide	109	1·18
Ether	74	0·71
Acetone	58	0·59
Methyl alcohol	32	0·33

VIII. *Third Confirmation of* Avogadro's *Law in its Application to Solutions.—Molecular Depression of Freezing-point of Solvent.*

Here, again, a general law may be stated, connecting osmotic pressure with the freezing-point of a solution:—*Solutions in the same solvent, and of the same freezing-point, are isotonic at that temperature.* This statement admits, like the former, of proof by means of a reversible cycle; but the solvent when returned is here in the condition of ice, not of vapour; the inverse change is also brought about by means of a semipermeable diaphragm, and, as it cannot be accompanied by gain or loss of energy, isotonism must exist.

We shall apply the above statement to dilute solutions; and applying the relations previously described, the simple conclusion follows that solutions containing an equal number of molecules in equal volume, and which therefore, from Avogadro's law, are isotonic, also have the same freezing-point. This law has been actually discovered by Raoult, and is expressed by him as "normal molecular depression of freezing-point." It holds for the great majority of dissolved substances examined, and consists in the statement that the depression of freezing-point of a one-per-cent. solution, multiplied by the molecular weight of the dissolved substance, gives a constant product; it is stated of solutions containing equal numbers of molecules in unit volume, assuming a close proportionality between concentration and lowering of the freezing-point. For an aqueous solution of nearly all organic bodies the constant product is about 18·5.

We can follow this relation still further, and, assuming Avogadro's law for solutions, deduce the above normal molecular depression of the freezing-point from other data. It bears a close relation to the heat of fusion of the solvent, as is shown by applying the second law of thermodynamics to a reversible cycle. Imagine a very dilute solution containing P per cent. of a dissolved substance, which has produced a depression of freezing-point, Δ; the solution itself freezes at T, and its heat of fusion is W per kilogram.

1. By use of piston and cylinder with semipermeable walls, the solution is deprived at temperature T of that amount of the solvent in which a kilogram-molecule of the dissolved substance existed; the amount of the solution is moreover so great that no appreciable change of concentration occurs, hence the work expended on it is 2T.

2. This quantity of solvent, $\frac{100\,M}{P}$, is then allowed to freeze at T by withdrawing $\frac{100\,MW}{P}$ calories; the solution and the solvent, now solid, are cooled through Δ degrees, and the solvent is now allowed to melt in contact with the solution, thereby absorbing the above quantity of heat. Finally, the temperature is raised Δ degrees.

During this reversible process $\frac{100\,MW}{P}$ calories rise from Δ to T, corresponding to an expenditure of energy equal to $\frac{100\,MW\Delta}{PT}$. In this expression, however, $\frac{M\Delta}{P}$ is the molecular depression of the freezing-point, which we may represent by the letter t; the work done is therefore $\frac{100\,Wt}{T}$, and it was shown above to be equal to 2T. Hence the equation

$$\frac{100\,Wt}{T} = 2T\text{; whence } t = 0{\cdot}02\,\frac{T^2}{W}.$$

This theoretical deduction receives ample confirmation from experimental data. The following table exhibits the molecular depression of freezing-point experimentally determined by Raoult (*Annales de Chimie et de Physique*, [5] xxviii.; [6] xi.), along with the values calculated by means of the above formula :—

Solvent.	Freezing-point (T).	Heat of fusion (W).	$t = \frac{0{\cdot}02\,T^2}{W}$.	Raoult's molecular depression.
Water . . .	273	79	18·9	18·5
Acetic acid .	273 + 16·7	43·2*†	38·8	38·6
Formic acid .	273 + 8·5	55·6*†	28·4	27·7
Benzene . .	273 + 4·9	29·1†	53	50
Nitrobenzene .	273 + 5·3	22·3†	69·5	70·7

It may be added that, from the molecular depression of ethylene bromide (117·9), the hitherto unknown heat of fusion is found equal to 13 ; and Mr. Pettersson, at my request, having experimentally determined this constant, obtained the number 12·94.

IX. *Application of* Avogadro's *Law to Solutions.—The Law of* Guldberg *and* Waage.

Having discussed the physical aspect of this problem in order to furnish as many proofs as possible of the correctness of its treatment, it now remains to apply it to chemical facts.

The most evident application of Avogadro's law in reference to solutions is to the determination of the molecular weights of dissolved substances. This has already been attempted ; but here it is not the pressure which requires measurement, as with gases, when the molecular weight is deducible from volume, pressure, and temperature ; with solutions the osmotic pressure must be measured, and as yet the practical method is wanting. Yet this difficulty may be avoided by substituting for a determination of pressure that of the related values ; as, for example, the diminution of vapour-pressure, or the depression of the freezing-point. This proposal is Raoult's. He divides the reduction of vapour-pressure of water containing one per cent. of dissolved substance into 0·185 parts, or the observed depression of freezing-point into 18·5 parts ; and this method is comparable in respect of accuracy with determinations of the density of gases, and is in itself a strong presumption of the accuracy of Avogadro's law in its application to gases.

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* Berthelot, *Essai de Mécanique Chimique.*

† Pettersson, *Journ. f. prakt. Chem.* [2] xxvi. p. 129.

It is still more remarkable that the law of Guldberg and Waage, so generally accepted for solutions, can be evolved from the above laws regulating the behaviour of dilute solutions. Again, a reversible cycle at constant temperature must be imagined, which can be conceived for solutions equally well with gases, by means of semipermeable diaphragms.

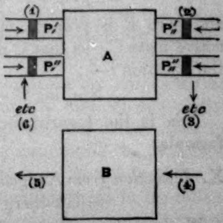
Let there be two systems of gaseous or dissolved substances in equilibrium, which may be expressed by the following symbols :—

$$a_{\prime}'\mathrm{M}_{\prime}' + a_{\prime}''\mathrm{M}_{\prime}'' + \&c. \rightleftarrows a_{\prime\prime}'\mathrm{M}_{\prime\prime}' + a_{\prime\prime}''\mathrm{M}_{\prime\prime}'' + \&c.,$$

where a denotes the number of molecules, and M the formula. This equilibrium exists in two different vessels, A and B, at the same temperature but at different concentrations, shown by the partial pressure, or by the osmotic pressure which each of the bodies exerts. Let these pressures be in vessel A, $\mathrm{P}_{\prime}'\mathrm{P}_{\prime}'' \ldots \mathrm{P}_{\prime\prime}'\mathrm{P}_{\prime\prime}''$ &c.; and in B greater than these by $d\mathrm{P}_{\prime}'$ $d\mathrm{P}_{\prime}'' \ldots d\mathrm{P}_{\prime\prime}'$ $d\mathrm{P}_{\prime\prime}'' \ldots$ &c.

The reversible cycle consists in introducing into A that amount in kilograms expressed by the first symbol of the first system, while the second is removed in equivalent amount: both systems have here the concentrations which exist in A (fig. 5). This change is accomplished by causing the entry or exit of each of the substances in question by means of its own cylinder and piston, which is separated from A by a diaphragm permeable to that body alone. If they are in solution, then the cylinders possess a semipermeable wall, and are surrounded by the solvent.

Fig. 5.



When this has been done, each constituent of the two systems has undergone such a change of concentration that it is now equal to that in B; and, as before, the work done per kilogram-molecule equals $2\Delta T$, where Δ represents a minute fraction of the pressure, and is here $\frac{dP}{P}$; for the amounts here in question it is $2aT\frac{dP}{P}$.

By making use of the vessel B, the second system, which has just been evolved, is now converted into the first, but of concentration as in B, proceeding as just described; and by suitable alteration of volume it is finally changed into its original state, as it at first existed in A.

As no change of temperature has occurred, the algebraic sum of all these operations is zero, as is seen from the numbers; it is thought unnecessary to interpret them, as they refer to the operations in the order in which they were carried out:— $(1)+(2)+(3)+(4)+(5)+(6)=0.$

Now (1) and (5) refer to operations opposite in sign on the same substances, with the same mass, at the same temperature; hence $(1)+(5)=0$. And for similar reasons, $(2)+(4)=0$; whence $(3)+(6)=0$.

This conclusion leads directly to Guldberg's and Waage's law.

The work expressed by (3) is from that law $\Sigma 2a_{\prime\prime}T\frac{dP_{\prime\prime}}{P_{\prime\prime}}$, and (6) is $\Sigma 2a_{\prime}T\frac{dP_{\prime}}{P_{\prime}}$; hence it follows that

$$\Sigma\left(2a_{\prime\prime}T\frac{dP_{\prime\prime}}{P_{\prime\prime}}-2a_{\prime}T\frac{dP_{\prime}}{P_{\prime}}\right)=0, \text{ or } \Sigma\left(a_{\prime\prime}\frac{dP_{\prime\prime}}{P_{\prime\prime}}-a_{\prime}\frac{dP_{\prime}}{P_{\prime}}\right)=0.$$

By integrating,

$$\Sigma(a_{\prime\prime}\log P_{\prime\prime}-a_{\prime}\log P_{\prime})=\text{constant};$$

and here P is proportional to the concentration, or to the active mass C; so that C may be substituted without altering the equation:—

$$\Sigma(a_{\prime\prime}\log C_{\prime\prime}-a_{\prime}\log C_{\prime})=\text{constant}.$$

This is the logarithmic form of Guldberg's and Waage's formula.

X. *Deviation from* Avogadro's *Law in Solutions.—Variations in* Guldberg *and* Waage's *Law.*

We have attempted to show the connexion between Guldberg and Waage's law and the laws of Boyle, Henry, Gay-Lussac, and Avogadro, as applied to liquids; as applied to gases, the truth of Guldberg and Waage's law has been long proved from thermodynamical considerations.

It remains to develop further the laws of chemical equilibrium, and, first, to investigate more closely the limits of applicability of the three fundamental principles from which Guldberg and Waage's law has been deduced.

So long as "ideal solutions" are under consideration, there exists strict analogy between gases and solutions; and just as there are deviations from Avogadro's law in the case of gases, so we may expect to find them with solutions. As, for example, the pressure of the vapour of ammonium chloride was found to be too great to be accounted for by Avogadro's law, so the osmotic pressure is in many cases abnormal; and as the high pressure in the first case is due to dissociation into ammonia and hydrogen chloride, it may be conceived that similar dissociation occurs in solutions. It must, indeed, be acknowledged that deviations are much more frequent with solutions than with gases, and occur often with bodies the dissociation of which, under ordinary circumstances, does not appear probable; in aqueous solution, for example, the majority of salts as well as the stronger acids and bases undergo dissociation; and hence Raoult did not discover the existence of so-called normal molecular depression of freezing-point and lowering of vapour-pressure until he investigated organic compounds; their behaviour is almost without exception regular. For these reasons it may have appeared daring to begin by giving prominence to Avogadro's law in its application to solutions; and I should have shrunk from this course had not Arrhenius pointed out to me the probability that salts, and analogous bodies, decompose on solution into their ions; in fact, substances which obey Avogadro's law are, as a rule, non-conductors, suggesting that in such cases no dissociation into ions occurs; and further experimental proof exists for other liquids, since by Arrhenius's assumption the deviations from Avogadro's law are calculable from the conductivity.

However this may stand, an attempt is made in the following pages to take account of such deviations from Avogadro's law, and, by help of the application of Boyle's and Gay-

Lussac's laws to solutions, to develop Guldberg's and Waage's formula so far as is possible.

The change in the expressions given above caused by these deviations is easily sketched.

The general expression for Boyle's, Gay-Lussac's, and Avogadro's laws, shown on p. 317, is

$$APV = 2T;$$

and this changes, if pressure is i-times that of this equation, into

$$APV = 2iT.$$

Hence, in a reversible cycle, the work will be i-times that previously done ; this alteration is easily applied to the former statement of Guldberg's and Waage's formula. Recurring to the final stage of the cycle described on p. 325,

$$(3) + (6) = 0,$$

the work corresponding to (3) and (6), which were formerly $\Sigma 2a_{\prime\prime}T\frac{dP_{\prime\prime}}{P_{\prime\prime}}$, and $-\Sigma 2a_{\prime}T\frac{dP_{\prime}}{P_{\prime}}$, is now increased i-times ; hence the equations become

$$\Sigma\left(a_{\prime\prime}i_{\prime\prime}\frac{dP_{\prime\prime}}{P_{\prime\prime}} - a_{\prime}i_{\prime}\frac{dP_{\prime}}{P_{\prime}}\right) = 0;$$

and on integration,

$$\Sigma(a_{\prime\prime}i_{\prime\prime}\log P_{\prime\prime} - a_{\prime}i_{\prime}\log P_{\prime}) = \text{constant}.$$

And introducing the concentration, or the active mass C, instead of the pressure proportional to it,

$$\Sigma(a_{\prime\prime}i_{\prime\prime}\log C_{\prime\prime} - a_{\prime}i_{\prime}\log C_{\prime}) = \text{constant}.$$

This is Guldberg's and Waage's law in a logarithmic form, differing from the former statement only by the introduction of the value i.

It remains to be shown that in this new form it agrees better with experimental results ; and as a knowledge of the correct value of i is necessary, we must deal with aqueous solutions, for sufficient experimental data are to be had only with such.

XI. *Determination of* i *for Aqueous Solutions.*

As Avogadro's law has been proved for solutions by four separate lines of argument, there are four ways in which the deviations, *i. e.* the values of i, may be determined. But that one which depends on the lowering of the melting-point has been so thoroughly proved experimentally that we shall confine ourselves to its use.

Reverting to the cycle which, on p. 323, was employed to prove the applicability of Avogadro's law to solutions, the relation was found :—

$$\frac{100\ \mathrm{W}t}{\mathrm{T}}=2\mathrm{T},$$

where the second term refers to the work done in removing or adding that amount of the solvent in which a kilogram-molecule of the substance was dissolved; that term must therefore be multiplied by i :—

$$\frac{100\ \mathrm{W}t}{\mathrm{T}}=2i\mathrm{T}.$$

In this manner a simple means of determining the value of i is apparent. The value of i is from the above equation proportional to t, *i. e.* to the molecular depression of temperature, for the other data (T=absolute melting-point, W=heat of fusion of solvent) are constant. Now 18·5 is the molecular depression for cane-sugar, which from p. 319 is seen to follow Avogadro's law accurately ; hence $i=1$; and for other bodies i is their respective depressions divided by 18·5. Almost identical results are arrived at by using in the above equation, instead of T and W, the values for ice, viz. 273 and 79 ; they will therefore be employed in the following calculations.

XII. *Proof of the Modified Law of* Guldberg *and* Waage.

In employing the relation proposed for the purpose of comparison with the results of Guldberg and Waage's formula, it is necessary to mention the different forms which it has assumed during the years since its discovery. We shall first give this relation a simple expression in which Guldberg and Waage's conceptions may be included, viz. :—

$$\Sigma ai \log \mathrm{C}=\mathrm{K}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

This differs from the one given on p. 328 merely in that the terms representing the components of both systems have had their signs changed. The original expression of the Norwegian investigators is closely analogous (*Christiania Videnskabs Selskabs Forhandlingar*, 1864):—

$$\Sigma k \log C = K \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

except that k has to be determined for each constituent in question by observation of the equilibrium of the system.

As Guldberg and Waage (*Études sur les affinités chimiques*, 1867) repeatedly found the coefficient k equal to 1, they simplified their equation thus:—

$$\Sigma \log C = K. \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In their last treatise, however (*Jour. für prakt. Chem.* xix. p. 69), they introduce the change that the number of molecules a must also be taken account of, and consequently they approximate their formula to that deduced for gases on thermodynamical grounds, thus:—

$$\Sigma a \log C = K. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

We have taken this last expression as their final formula.

The Norwegian investigators maintained this simple formula, with whole numbers as coefficients, even for solutions; but Lemoine has lately revived the original formula (2) in order to represent the results of Schlösing's investigations on the solubility of calcium carbonate in water containing carbon dioxide, with constants hereafter to be determined, which are, however, not whole numbers; for if whole numbers be employed, it is impossible to reconcile fact with theory.

In view of this uncertainty, the formula which we have suggested has this advantage, that the coefficients which occur in it are *à priori* determined, and its truth can at once be decided by experiment. It will in fact appear that in the instances studied by Guldberg and Waage the simple form which they recommend is completely confirmed; and that such simplification is in most cases admissible confirms, moreover, the validity of Avogadro's law in its application to solutions. On the other hand, the results of Schlösing's investigation, prominently alluded to by Lemoine, cannot be simply expressed, and require the same fractional coefficients which Schlösing himself arrived at.

Before proceeding to this investigation, it is necessary to include cases where partially insoluble bodies are present:

this is easily done; they may be included in the formulæ given above, remembering that such bodies exist in solution up to its saturation-point, and are therefore present with constant concentration. All concentrations depending on this may, therefore, be removed from the first term of the above equation to the second, without affecting the constancy of the latter. Everything remains the same, except that in the first term only the dissolved bodies need be considered.

1. We shall first examine Guldberg and Waage's observations. The first case they studied was that expressed by the equation

$$BaCO_3 + K_2SO_4 \rightleftarrows BaSO_4 + K_2CO_3 ;$$

and they found, according to their simplified formula, that

$$\log C_{K_2SO_4} - \log C_{K_2CO_3} = K.$$

The relation given by our equation is almost identical, for, for K_2SO_4, $a=1$ and $i=2{\cdot}11$, and for K_2CO_3, $a=1$ and $i=2{\cdot}26$; hence

$$\log C_{K_2SO_4} - 1{\cdot}07 \log C_{K_2CO_3} = K.$$

A similar agreement exists with sodium carbonate, for then the values of i for Na_2SO_4 and Na_2CO_3 are 1·91 and 2·18 respectively; hence

$$\log C_{Na_2SO_4} - 1{\cdot}14 \log {}_{Na_2CO_3} = K.$$

2. This result, expressed in what is almost a whole number, cannot be expected in the above-mentioned experiment of Schlösing (*Comptes rendus*, lxxiv. 1552; lxxv. 70). There the subject of experiment was the solubility of calcium carbonate in water containing carbonic acid, and the state of equilibrium is expressible by the following statement:—

$$CaCO_3 + H_2CO_3 \rightleftarrows Ca(HCO_3)_2.$$

We should expect that, as $i=1$ for carbon dioxide, that i should $=2{\cdot}56$ for calcium hydrogen carbonate:—

$$0{\cdot}39 \log C_{H_2CO_3} - \log C_{Ca(HCO_3)_2} = K ;$$

and Schlösing found:—

$$0{\cdot}37866 \log C_{H_2CO_3} - \log C_{Ca(HCO_3)_2} = K.$$

Similar experiments with barium are equally satisfactory;

the value of *i* for barium hydrogen carbonate is 2·66, and the following results are calculated :—

$$0{\cdot}376 \log C_{H_2CO_3} - \log C_{Ba(HCO_3)_2} = K.$$

The experimental result is

$$0{\cdot}38045 \log C_{H_2CO_3} - \log C_{Ba(HCO_3)_2} = K.$$

3. Turning to Thomsen's investigation on the action of sulphuric acid on sodium nitrate in solution (Thomsen's *Thermochemische Untersuchungen*, i.), we find a similar agreement.

The reaction may be formulated thus :—

$$Na_2SO_4 + HNO_3 \rightleftarrows NaHSO_4 + NaNO_3\,;$$

and Guldberg and Waage's relation requires

$$\log C_{Na_2SO_4} + \log C_{HNO_3} - \log C_{NaHSO_4} - \log C_{NaNO_3} = K.$$

Now

$i_{Na_2SO_4} = 1{\cdot}91$; $i_{HNO_3} = 1{\cdot}94$; $i_{NaHSO_4} = 1{\cdot}88$; and $i_{NaNO_3} = 1{\cdot}82$; and the equation becomes

$$1{\cdot}05 \log C_{Na_2SO_4} + 1{\cdot}06 \log C_{HNO_3} - 1{\cdot}03 \log C_{NaHSO_4} - \log C_{NaNO_3} = K,$$

which is almost identical.

If we express the relation thus :—

$$Na_2SO_4 + 2HNO_3 \rightleftarrows H_2SO_4 + 2NaNO_3,$$

Guldberg and Waage's formula becomes

$$\log C_{Na_2SO_4} + 2 \log C_{HNO_3} - \log C_{H_2SO_4} - 2 \log C_{NaNO_3} = K\,;$$

and we obtain :

$$\log C_{Na_2SO_4} + 2{\cdot}03 \log C_{HNO_3} - 1{\cdot}07 \log C_{H_2SO_4} - 1{\cdot}91 \log C_{NaNO_3} = K.$$

Again an almost absolute agreement.

4. Ostwald's investigation (*J. prakt. Chem.* [2] xix. p. 480) on the action of hydrochloric acid on zinc sulphide, according to the formula

$$ZnS + 2HCl \rightleftarrows H_2S + ZnCl_2,$$

leads, when the following values are ascribed :

$$i_{HCl} = 1{\cdot}98\,;\; i_{H_2S} = 1{\cdot}04\,;\; i_{ZnCl_2} = 2{\cdot}53$$

to the equation :—

$$3{\cdot}96 \log C_{HCl} - 1{\cdot}04 \log C_{H_2S} - 2{\cdot}53 \log C_{ZnCl_2} = K.$$

It is evident that the concentrations of the sulphuretted hydrogen and the zinc chloride are equal, for only hydrochloric acid and zinc sulphide are initially present. The result is expressible by taking the volume V, in which a known amount of hydrochloric acid was dissolved, as the initial concentration; and the fraction x to denote that portion which after contact with zinc sulphide has finally reacted, forming zinc chloride. Hence we have

$$3{\cdot}96 \log \frac{1-x}{V} - 3{\cdot}57 \log \frac{x}{V} = \text{const.};$$

and also

$$\frac{x}{(1-x)^{1{\cdot}11}} V^{0{\cdot}11} = \text{const.}$$

This function is really nearly constant:

Volume (V).	Portion reacted (x).	$\frac{x}{(1-x)^{1{\cdot}11}} V^{0{\cdot}11}$.
1	0·0411	0·043
2	0·038	0·0428
4	0·0345	0·0418
8	0·0317	0·0413

Similar experiments with sulphuric acid, where i for H_2SO_4 and $ZnSO_4$ is respectively 2·06 and 0·98, gave

$$\frac{x}{(1-x)^{1{\cdot}02}} V^{0{\cdot}02} = \text{const.};$$

and here also x is nearly a constant, as is seen from the following experimental numbers:—

Volume V.	Portion reacted (x).
2	0·0238
4	0·0237
8	0·024
16	0·0241

5. The experiments of Engel on the solubility of magnesium carbonate in water containing carbonic acid (*Comptes rendus*, c. pp. 352, 444) also deserve notice. The state of equilibrium is

$$MgCO_3 + H_2CO_3 \rightleftarrows Mg\,(HCO_3)_2\,;$$

and our formula leads to the following relation, where $i = 2{\cdot}64$ for acid magnesium carbonate:—

$$0{\cdot}379 \log C_{H_2CO_3} - \log C_{Mg(HCO_3)_2} = K,$$

The number found was

$$0{\cdot}370 \log C_{H_2CO_3} - \log C_{Mg(HCO_3)_2} = K.$$

6. Other experiments by the same author on the simultaneous solubility of copper and ammonium sulphates (*Comptes rendus*, cii. p. 113), are shown by the reversible equation

$$CuSO_4 + (NH_4)_2SO_4 \rightleftarrows Cu(NH_4)_2(SO_4)_2.$$

As the double salt was always partly undissolved, and as the values of i for $CuSO_4$ and for $(NH_4)_2SO_4$ are 0·98 and 2·0, we have the relation

$$0{\cdot}49 \log C_{CuSO_4} - \log C_{(NH_4)_2SO_4} = K.$$

The number found is $0{\cdot}438 \log C_{CuSO_4}$.

7. In conclusion, we may notice Le Chatelier's experiments on the equilibrium between basic mercuric sulphate and sulphuric acid (*Comptes rendus*, xcvii. p. 1565);

$$Hg_3SO_6 + 2H_2SO_4 \rightleftarrows 3HgSO_4 + 2H_2O.$$

In this case the values of i for H_2SO_4 and $HgSO_4$ are 2·06 and 0·98, and we have

$$1{\cdot}4 \log C_{H_2SO_4} - \log C_{HgSO_4} = K.$$

The found value is $1{\cdot}58 \log C_{H_2SO_4}$.

Generally speaking, therefore, such results show a very satisfactory agreement.

Prof. Rücker regretted that the names Boyle's Law and Gay-Lussac's Law had been so persistently made use of in the paper, as he thought a wrong impression would be spread as to the nature of the phenomena. He also considered it probable that the proportionality observed was merely the result of the smallness of the ranges over which the experiments had been made. Mr. H. Crompton took exception to the imaginative character of the reasoning, and thought much more experimental proof was required before the results could be accepted for any but very small ranges of concentration. In answer to Prof. Reinold, Prof. Ramsay said the experimental data were not obtained by Van't Hoff himself, but were taken chiefly from Raoult's determinations.

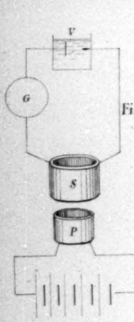


Fig. 2

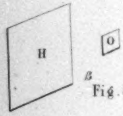
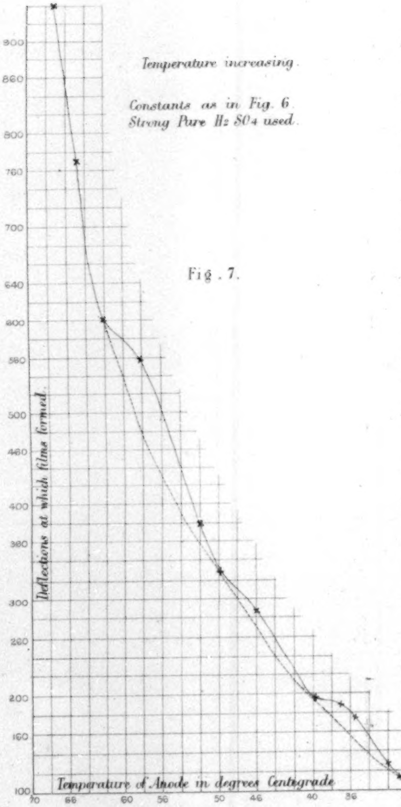


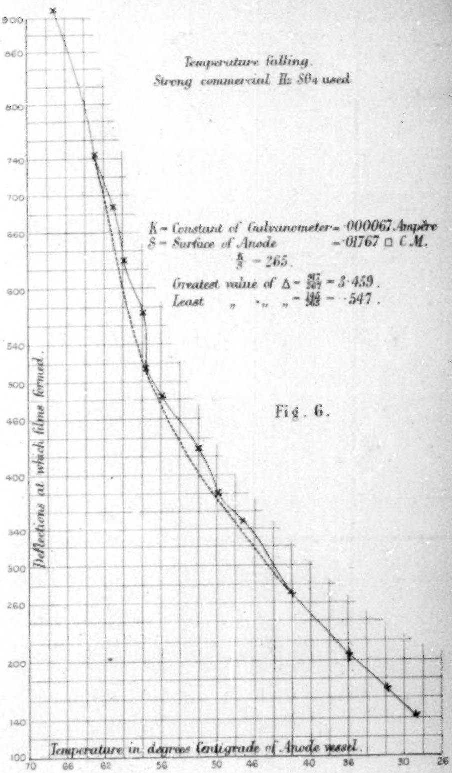
Fig. 5.



Temperature increasing.

Constants as in Fig. 6.
Strong Pure H_2SO_4 used.

Fig. 7.



Temperature falling.
Strong commercial H_2SO_4 used

K = Constant of Galvanometer = ·000067 Ampère
S = Surface of Anode = ·01767 □ C.M.

$\frac{K}{S} = 265.$

Greatest value of $\Delta = \frac{917}{265} = 3{\cdot}459.$
Least " " " $= \frac{145}{265} = {\cdot}547.$

Fig. 6.

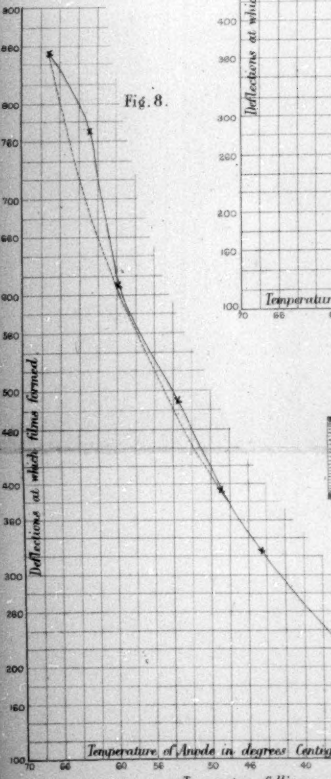


Fig. 8.

Temperature falling.

Constants as before

Strong Pure H_2SO_4 used.

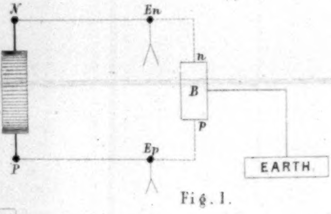

N
P
En
Ep
n
B
p
EARTH

Fig. 1.

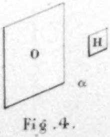


Fig. 4.

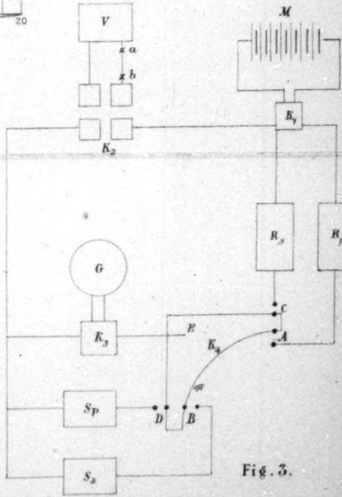

V
a
b
M
K_1
K_2
R_s
R_p
G
K_3
E
K_4
C
A
D
B
S_p
S_s

Fig. 3.

Mintern Bros. lith.

XLV. *Experiments on Electrolysis.*—Part II. *Irreciprocal Conduction.*

By W. W. Haldane Gee, *B.Sc., Lecturer of the Victoria University, and* H. Holden, *B.Sc., Bishop Berkeley Fellow in Physics, of the Owens College, Manchester*[*].

[Plate X.]

We have observed, when strong sulphuric acid is used as an electrolyte, the electrodes being of platinum, that the decomposition nearly ceases, if by decreasing the resistance in circuit we attempt to increase the current beyond a certain maximum. An investigation of this curious behaviour has shown that it may best be included with the miscellaneous phenomena grouped under the head of *unipolar*, or, as it may better be called, *irreciprocal* conduction[†]. The early experimenters in voltaic electricity devoted much attention to this subject, and it will be desirable to give some account of their labours before proceeding to describe our own experiments.

Section A. Historical and Critical.

1. *Experiments of Erman.*

Among the many papers in the earlier volumes of Gilbert's *Annalen der Physik*, which the invention of the voltaic pile had directly inspired, will be found several by Prof. Erman of Berlin. With the aid of the imperfect instruments known at that time he made an investigation on the conducting power of bodies; and in his fourth research[*] there appears a division of bodies into five classes, of which the following schema shows the chief features :—

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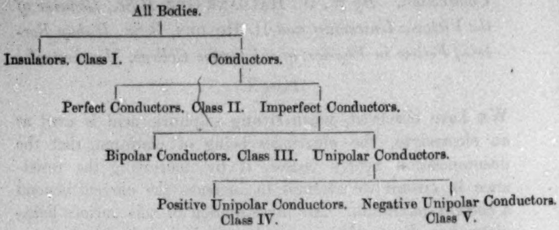
[*] Read May 26, 1888.

This is the second of a series of papers on Electrolysis and Electrolytic Polarization, descriptive of experiments made since January 1887 at the Owens College Physical Laboratory. An abstract of the experiments made, to the end of August 1887, was submitted to the British Association meeting at Manchester. We desire to acknowledge the assistance received up to that time from Mr. C. H. Lees, B.Sc., Derby Mathematical Scholar of the Owens College. His cooperation has since been discontinued, owing to absence at Strasburg.

[†] Following Christiani's use of the term, irreciprocal conduction is said to occur if a reversal of the *direction* of a current causes any change in its *magnitude*.


All Bodies.
Insulators. Class I.
Conductors.
Perfect Conductors. Class II.
Imperfect Conductors.
Bipolar Conductors. Class III.
Unipolar Conductors.
Positive Unipolar Conductors. Class IV.
Negative Unipolar Conductors. Class V.

To understand this classification it will be necessary to describe the method of experiment employed by Erman in order to find the class to which a body belonged. PN (Pl. X. fig. 1) is a well insulated Volta's pile, P and N being the positive and negative poles respectively. Connected with P is a gold-leaf electroscope E_p and with N a similar electroscope E_n. The body (B) to be tested, which is permanently connected with earth, is made to touch separately E_p and E_n. If the body belongs to Class I. neither of the electroscopes will be affected, but the case is otherwise if B is a conductor. Let us suppose the latter; it will, therefore, be necessary to ascertain to which of the other four classes the body belongs. Permanent connexions are made (as represented by the dotted lines of fig. 1) with B, and the behaviour of the electroscopes is carefully noted. If there is a sudden and complete collapse of the leaves of both E_p and E_n, the body belongs to Class II.[†]; if, on the other hand, both E_p and

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[*] "Ueber die fünffache Verschiedenheit der Körper in Rücksicht auf galvanisches Leitungsvermögen." Gilbert's *Annalen der Physik*, Band xxii. S. 14 (1806).

[†] The following extract from Erman's paper (*loc. cit.* p. 18) is interesting, as evidence of his scientific caution:—"The distinction between perfect insulators and perfect conductors was perceived very early with the battery, and though it may indeed be believed that no important discoveries remain to be made in this matter, it is very desirable not to consider the matter as fully settled, for it is possible that also here important modifications of conductivity lie hidden. For example, it has really not

E_n are partially discharged, B is placed in Class III. If E_p alone is discharged, B is a positive unipolar conductor; if this is the case only with E_n, B is a negative unipolar conductor.

At the time when Erman classified conductors in this manner Ohm's law was not known, hence the behaviour of the electroscopes under these conditions could not be thoroughly appreciated. However, it was recognized that in the cases of Classes IV. and V. there must be a high resistance at one of the contact points n or p, preventing the conduction of the charge of E_n or E_p to earth.

It was found that well-dried soap was a good example of Class V. Hence Erman studied the unipolar conducting-power of soap in some detail, but he was unable to come to any definite conclusion why at p there should be a very high resistance. He rejected, on grounds which need not here be described, the hypothesis of an insulating film composed of the decomposed soap-acids *.

Interesting examples of Class IV., Erman found, are furnished by flames. Their anomalous conductivity yet remains but imperfectly explained †.

2. *Experiments of Ohm.* (a) *On Soap.*

Ohm ‡ confirmed the experiments of Erman, and came to the conclusion that the cause of the high resistance at the anode was due to a layer of fatty acids.

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yet been proved whether, with the so-called perfect conductors, there are not degrees of conductivity. Thus, though it is usual to regard all metals as equally good conductors, I should like this law not to be so unconditionally assumed, before direct measurements have proved its generality. Indeed, who knows whether finally with each metal certain properties will not be made apparent, which in respect to galvanic electricity are analogous to those which so excellently characterize iron, nickel, and cobalt with regard to magnetism?"

* *Loc. cit.* p. 50.

† The conducting power of flames has been studied by Ritter, Hittorf, Andrews, Hankel, Braun, Fuchs, Macfarlane, Rintoul, and others.

‡ Schweigger's Journal, lix. (1830) p. 385.

(b) *On Sulphuric Acid and Caustic Potash.*

Ohm[*] employed a galvanometer in studying the unipolar conductivity of these liquids. The instrument was placed in series with 1–6 single fluid zinc-copper cells, and a voltameter containing strong sulphuric acid. Employing electrodes of platinum, gold, silver, copper, iron, lead, and tin, he found that the deflection obtained on first making the circuit speedily sank to a very small amount. He showed that the cause diminishing the current was between the anode and the acid; for on bridging across the anode and the liquid by some moist threads the deflection was immediately increased to the original amount. Also, in the case of the oxidizable metals, by diluting the liquid in the neighbourhood of the anode the current was greatly increased, whilst a dilution of the liquid near the kathode produced no effect. Further, replacement of the anode by a clean electrode produced the original deflection for a time, whilst changing the kathode made no difference. He finally concludes that the fall of the current is due to the development of a force at the anode. This conclusion he verified by the use of an electroscope. Thus strong sulphuric acid is classed amongst the negative unipolar conductors[†].

Ohm also experimented with various electrodes in strong potash solution, which led him to regard the latter as a positive unipolar conductor.

3. *Experiments and Theory of Christiani.*

The only modern contribution of importance to the subject of irreciprocal conduction is the pamphlet[‡] produced by the late Prof. Christiani, whilst working in the laboratory of Du Bois Raymond. He investigated generally the case of electrolysis with unequal-sized electrodes, and comes to the conclusion that, as a rule, with low E.M.F.'s the current is able to pass more easily from the lesser to the greater electrode, and *vice versâ* with large E.M.F.'s. This difference, he says, exists after the influences of polarization and chemical transition resistance are allowed for, meaning by the latter term a resistance due to the products of electrolysis. We have been at some trouble to examine the experiments and reasoning upon which the above conclusion is based, and have arrived at the opinion that they are equally open to criticism; his results and theory being, however, very interesting and representing a large amount of work, it will hence be desirable to notice them at some length, especially as his pamphlet is not readily accessible in England.

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[*] Schweigger's Journal, lx. (1830) p. 32.

[†] It seems probable that what Ohm observed was a decrease of current due—(1) in the case of the oxidizable electrodes to a transition resistance caused by the formation of a badly conducting salt-layer on the anode in strong sulphuric acid; (2) in the case of platinum and gold in sulphuric acid and potash solution to polarization, this polarization being of a unipolar nature, that is to say it was much greater at one electrode than at the other. We do not believe that Ohm ever observed the phenomenon of unipolarity in strong sulphuric acid with electrodes of platinum or gold due to a transition resistance. This conclusion is supported by comparing Wiedemann, *Electricität*, Band ii. pp. 629, 657.

[‡] *Ueber irreciproke Leitung electrischer Ströme.* Berlin: Friedländer und Sohn (1876). Contains 174 pages and 15 plates.

In order to eliminate as much as possible changes in the electrolyte caused by the passage of the current, momentary currents from an induction-coil were used throughout the greater part of his work, his object being a study of the nature and causes of irreciprocal conduction, with unequal-sized electrodes, in electrolytes. For this purpose, applying a momentary E.M.F. to the voltameter, he desires to measure:—

(*a*) The quantity of electricity which is sent through the voltameter by this E.M.F., and (*b*) the polarization which opposes its passage.

Fig. 2 shows the essential parts of the arrangement used with induced currents. V is the voltameter with a platinum plate and a platinum Wollaston's point as electrodes. In circuit with it are a dead-beat Wiedemann's galvanometer (G) and the secondary coil (S) of an induction-coil, of which P is the primary coil in circuit with a battery. By changing the distance of S from P ($=x$), the E.M.F. of the induced current can be varied. The experiments were conducted somewhat as follows:—By opening the primary circuit a quantity of electricity ($=q_1$) is sent through the voltameter, and the kick ($=k_1$) on G is noted. If the deflection does not return exactly to zero, but goes on the other side of it, this negative deflection ($=e_1$) is observed, and afterwards, in discussing the possible causes of irreciprocal conduction, is taken as a measure of the polarization opposing the passage of q_1. The same induced E.M.F. is now, by reversing the voltameter terminals, applied in the opposite direction. Let k_2 and e_2 be the deflections now obtained, and suppose that q_2 is the quantity of electricity sent through the voltameter in this direction of the current. The distance of S from P is now altered, generally by 5 millim. at a time, and at each new position the above procedure is repeated, giving values of k_1 and k_2 for each value of x. He finds that k_1 and k_2 are generally different, but for *one* value of x they are equal; for values of x below this, one of them is the greater; for values of x above this, the other is the greater. He then assumes that k_1 and k_2 are proportional to q_1 and q_2 respectively, and thus proves the existence of irreciprocal conduction.

The assumptions made, namely (1) that e_1 is a measure of the polarization E.M.F. which opposes the passage of q_1, and (2) that k_1 is exactly proportional to q_1, do not seem to be admissible. Firstly, with regard to (1), e_1 is really proportional to the depolarization current which exists a certain time, conditioned by the time of vibration of the galvanometer &c., after the charging E.M.F. has ceased, and is not proportional to the value of the polarization E.M.F. during the existence of the charging E.M.F., unless the rate of depolarization is constant, which it usually is not. Also, as will be shown in a subsequent paper, the rate of depolarization with unequal-sized electrodes depends on the direction of the charging current. It may also be noticed that q_1 or q_2 &c. do not necessarily bear any simple relation to the value of the induced E.M.F. The quantity of electricity which passes in any time dt during the existence of the induced E.M.F. is proportional to the difference between the values at that time of the induced E.M.F. and of the polarization; and therefore the relation between the charging E.M.F. and the quantity of electricity which passes during its existence depends on the rate of increase of the polarization E.M.F., which can scarcely be said to be known, but is probably not constant, and may even vary according to the direction of the current. The validity of assumption (2) is also doubtful, for the kick (k) on the galvanometer depends not only on q (the quantity of electricity which passes during the existence of the induced

E.M.F.) but also on the magnitude of the depolarization current, since the duration of the induced E.M.F. is certainly over before the galvanometer-needle has moved from zero to *k* divisions. Thus it would seem that *k* is only at best, in the case of non-polarizable electrodes, an approximation to the value of *q*; and therefore Christiani's numbers are not sufficiently trustworthy to serve as foundation for an elaborate theory.

Christiani, however, assumes that *k* and *q* are exactly proportional and plots curves* giving the relation between *q* and the induced E.M.F. in the two directions of the current. The point of intersection of the two curves is called the reciprocal point; its abscissa being proportional to the induced E.M.F., which gives equal kicks in the two directions, and its ordinate equal to these kicks. The conditions for the occurrence of reciprocity are then investigated. He finds (1) that lessening the size of the point electrode causes reciprocity to occur with lower E.M.F.'s, but that fairly large changes in the size of the plate electrode or in the distance of the electrodes have not much influence; (2) that decreasing the temperature causes reciprocity to occur with lower E.M.F.'s; and (3) that the degree of concentration of the solution is of influence. A discussion of the possible causes of irreciprocal conduction follows. Polarization, he says, cannot be the cause of irreciprocity, because he has obtained the latter with non-polarizable electrodes. Further, some measurements of the polarization by a wippe method show, as far as such a method is satisfactory †, that polarization is not the main cause of irreciprocity. His arguments against polarization, based on the measurements of e_1 (see p. 339) are, as before explained, of no value. His main argument against a chemical transition resistance is the following experiment:—A solution of potassium iodide is taken, and a certain number of currents sent through until the E.M.F. is found for which reciprocity occurs; the electrodes are then taken out and cleaned, and fresh solution used. The same E.M.F. is now again applied, and it is found that reciprocity still occurs; or, in other words, changes in the electrolyte caused by the passage of previous currents do not affect the value of the E.M.F., for which the momentary currents are the same in the two directions. The direction in which the E.M.F. is first applied is also quite indifferent. He remarks, however, that the reciprocal point in other electrolytes is, to some extent, affected by the passage of previous currents.

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* These curves are regular and never intersect in more than one point, and with some electrolytes do not intersect within the limits of experiment.

† The values of the polarization obtained by this method are of course open to the objection that they depend on the rate of fall of polarization after the removal of the charging battery.

Christiani to a small extent makes use of permanent currents, but only in the case of the so-called non-polarizable electrodes. The following specimen of his results, with amalgamated zinc electrodes in concentrated zinc sulphate, will indicate their general features :—

1st Experiment.—Plate electrode $= P_p = 37 \times 26$ millim.
Point electrode $= P_s = \frac{1}{2}$ size of P .

Seconds after making circuit.	Deflections.	
	P_s+.	P_s-.
0	217	217·5
120	Begins to fall.	
600	45, vibrating.	220

2nd Experiment.—P_p = same size as before. $P_s = \frac{1}{4}$ size of P_p.

Seconds after making circuit.	Deflections.	
	P_s+.	P_s-.
0	188·0	196·0
40	Begins to fall.	
180	14·0, vibrating.	
600		196·0

Here it seems evident that the irreciprocity is due to the gradual formation of a badly-conducting film on the anode, but Christiani does not accept this simple explanation, apparently because it would not, as he thinks, explain the results obtained with transient currents.

Having rejected both polarization and a chemical transition resistance as causes for the effects obtained, a theory of electrolysis which would afford an explanation of them was necessary. The theory which Christiani evolves is a direct development of the idea that electrolysis is a convection of electricity by the atoms of matter. He considers the mechanism of electrolysis to consist in the two ions of a molecule conveying equal and opposite quantities of electricity in opposite directions, but assumes (1) that *the impulse causing this motion takes place only at the anode.* He assumes, moreover, (2) that in a given electrolyte the number of atoms $(=n)$ which meet any unit area in unit time only depends on the temperature of the electrolyte. Finally, he makes use of the assumption (3) that every monad ion carries the same quantity of electricity $(=e)$. These assumptions are applied finally to the case of unequal-sized electrodes, a plate of area P and a point of area S, as follows. According to assumption (2) the number of atoms which touch the plate in unit time will be $n\mathrm{P}$, and therefore by (3) the greatest quantity of electricity which can leave the plate by ordinary convection in unit time is $n\mathrm{P}e$, and similarly $n\mathrm{S}e$ is the greatest quantity of electricity which can leave the point in unit time. He then argues that, although n is a very large quantity, e is a very small one, and that it may thus happen that $n\mathrm{P}e$ and $n\mathrm{S}e$ are quantities of electricity small enough to be dealt with in experiment. He then considers the effect of an E.M.F. $(=\mathrm{E})$ acting in the circuit for a very short time $(=t)$. Suppose that the plate is the anode and let such an E.M.F. $(=\mathrm{E}_p)$ be applied for a very short time $(=t)$ that the quantity of electricity which passes is equal to $n\mathrm{P}et$. In this case, when the E.M.F. applied is just sufficient to charge the $n\mathrm{P}t$ atoms which appear at the plate anode during the time t of charge, Christiani says, from energy considerations, that the most favourable conditions for the passage of the electricity are attained, or, in other words, the resistance of the cell is now a minimum. If the E.M.F. applied be less than E_p the resistance of the cell increases because all the atoms which touch the anode are not engaged in carrying the electricity; if, on the other hand, an E.M.F. greater than E_p is used, the resistance of the cell again increases because a process analogous to spark-discharge has to go on. Similarly, if the point is the anode, there will be a certain E.M.F. ($=E_s$) for which the resistance of the cell, for the direction of the current, is a minimum $\left(=\frac{E_s}{nSe}\right)$. Let the E.M.F. actually applied be E, there are then three cases possible. Since $E_s < E_p$,

$$(1)\ E < E_s < E_p, \quad (2)\ E_s < E < E_p, \quad (3)\ E_s < E_p < E.$$

Now, according to Christiani, the resistance of the cell is greater or less (*i. e.* an additional *electrical* transition resistance is greater or less) according as the E.M.F. applied is more or less different in value from that giving the minimum resistance. Thus in case (1), where E is nearer to E_s, the quantity of electricity which passes will be greater if the point is the anode. In case (2), where E is between E_s and E_p, there will be one value of E which will give equal quantities of electricity in either direction of E. In case (3), where E is nearer to E_p, the quantity of electricity will be greater if the plate is the anode.

The theory is thus highly ingenious, but its verification is a matter of considerable difficulty, for we have to distinguish the effects of polarization and *chemical* transition resistance from the *electrical* transition resistance. As we have pointed out, Christiani's own results are open to doubt, so that unless some better method of experiment can be suggested the hypothesis of Christiani must remain but an interesting speculation.

Section B. Descriptive of our Experimental Work.

1. *Arrangement of Apparatus.*

We have found the arrangement shown in fig. 3 very convenient for the larger part of our experimental work.

M is a battery consisting usually of from 1 to 6 secondary cells;

k_1 is a commutator for reversing the main current;

k_2 is a four-way plug-key enabling the voltameter-circuit to be broken or short-circuited, or cut out of the circuit altogether;

V is the electrolytic cell;

G is a dead-beat Deprez and D'Arsonval galvanometer;

k_3 is a commutator used to reverse the current through G;

k_4 is a switch hinged at E. According as the contacts D and A *or* those at C and B are made, then (1) the battery is included *or* excluded from the voltameter-circuit, (2) the low-resistance galvanometer-shunt S_p *or* the high resistance galvanometer-shunt S_s is in use, and (3) the resistance-box R_p *or* R_s is employed. The switch k_4 enables us, therefore, to study the changes of current-intensity when the electrolytic cell is in circuit with the battery, and also the changes of the polarization-current which ensue after cutting the battery out of the circuit.

2. *Experiments with strong* H_2SO_4 *and* Pt *Electrodes.*

a. *General description of the Phenomenon to be observed.*—In the opening paragraph of this paper it was stated that, through a given voltameter, consisting of Pt electrodes in strong H_2SO_4, currents of an intensity below a certain maximum could alone pass, and that if we tried to increase the current strength above this maximum, it was found that the deflection on a galvanometer in circuit diminished nearly to zero, and that decomposition nearly ceased. Further investigation soon showed that the size of the electrodes was also of great influence in determining the stoppage of the current, as is well shown by the following arrangement (see also Table I.). Take two clean platinum electrodes, *one being much larger than the other*, and place them in strong pure H_2SO_4. It will be found that a very large current can pass from the larger to the smaller electrode (see fig. 4, α direction of current), giving a deflection say of several hundred divisions on the galvanometer. On reversing the current (see fig. 5, β direction of current), if it be of suitable strength, after the first sudden deflection the galvanometer-needle will soon return nearly to zero; in many cases scarcely a trace of a deflection is to be observed; in fact the current in the α direction may be more than a thousand times as great as that obtained in the β direction on reversing k_1. We desire to ascertain the cause of this (almost total) stoppage of the current, and to study generally the conditions determining its occurrence; and in connexion with this, it may be noted that the formation of a sheath of gaseous bubbles round the anode always attends this stoppage of the current with Pt electrodes.

b. *Experiments proving Existence and Locality of an Insulating Film.*—If, after the production of the insulating condition, we replace the kathode by a *large* electrode the current is still stopped, but if the anode be similarly exchanged the current passes readily. This experiment proves that there is some obstructing cause at the anode. To determine its nature attempts have been made to measure the polarization and resistance of the cell in the α and β directions. Operating very rapidly with the dead-beat galvanometer, and using Ohm's method, we find that the resistance in the β direction (after the insulating condition is produced) is very much greater than in the α direction. Thus in one experiment the resistance in the α direction was only 10 ohms, whereas on reversal it rose to 10,000 ohms, and continued to increase for some time after the insulating condition was produced. The polarization, on the other hand, may be regarded as equal in the two directions, for, using a compensation method in which the voltameter-circuit was momentarily opened, and the polarization balanced against a known E.M.F., we have found 2·5 volts to be the value both when the insulating condition was produced, and also when it was not. This result was confirmed by Ohm's method. Thus the polarization is not abnormal, and therefore cannot be a determining factor in producing the insulating condition. Such a conclusion was from the first to be expected, as the stoppage of the current had been produced when a battery having an E.M.F. of ten volts and upwards had been employed. Measurements of the resistance were also taken by Kohlrausch's method, this being done by breaking the circuit of fig. 3 between *a* and *b*, and including the arrangement in one of the arms of a Wheatstone's bridge. It was found very difficult to get anything but a very approximate value of the resistance during the insulating condition, for the passage of the alternate currents apparently tended to destroy the insulation, and caused continual vibration of the needle of the dead-beat galvanometer. This effect was much less when the current was sent in one direction through the primary coil of the induction-coil than in the other. The values of the resistance obtained, however, fully confirmed those obtained by other methods; so that we may conclude that the insulating condition is due to a resistance at the anode. In what follows it will be convenient to regard this as due to a film of badly-conducting matter.

c. *The Film-forming Current-density.*—If we increase the size of the anode a greater current is necessary to produce the insulating condition, hence the current-density at the anode is an important factor in determining the formation of the film (see Table I.). Calling C the minimum current which will form the film and S the whole surface of the anode immersed, then $C/S=\Delta$ we shall define as the *film-forming current-density*. The actual value of Δ depends upon several conditions, such as will be best understood after an examination of Tables I. and II.

Examination of the following and similar observations show that Δ is not perfectly definite in value, but is greatly influenced by the previous treatment of the anode. For example, if the anode has been heated to redness and washed before putting in the current, the film forms with a less value of Δ than if the anode had been previously used for electrolysis (see experiments 2 and 3, Table I., and 5, 6, 7, and 21, Table II.). Generally speaking, previous currents in either direction cause Δ to increase, but after the current has been on for some time in the β direction, a reversal for a short period seems to assist the formation of the film. Experiment 6, Table I., shows very markedly the effect of unclean electrodes. It is noteworthy that, when the current-density in the β direction is at first of a value Δ' greater than Δ, the rapidity and completeness of the film-formation depends upon $\Delta'-\Delta$.

TABLE I.—Showing Effect of (1) Size of Anodes, (2) Previous history of Electrodes.

k=constant of galvanometer=·000365 ampere.
S=surface of anode=l × ·06 sq. cm. approximately.
l=length of anode.

The electrodes used were of platinum, one of them being a plate 40 millim. × 8·5 millim., the other a wire ·2 millim. diameter, whose length was varied during the experiment. These were placed in strong pure H_2SO_4, and a battery with an E.M.F. of 6 volts employed.

Remarks.	l.	C.	Δ.
	mm.	ampere.	ampere per sq. cm.
1. Had been passing current a long time in β direction	20	k 287=·105	·87
2. Ditto	10	k 140=·0511	·85
3. Washed and heated electrodes	"	k 100=·0365	·61
4. Current on in α direction for 15 seconds	"	k 90=·0328	·55
5. Ditto for 60 seconds	"	k 92=·0335	·56
6. Wiped electrodes with paper. Shows influence of unclean electrodes	"	k 240=·0876	1·46
7. Washed and heated electrodes	"	k 110=·0402	·67
8. Current on in α direction for 30 seconds	"	k 150=·0547	·91
9. Ditto for 45 seconds	"	k 120=·0438	·73
10. Broke circuit; lifted electrodes up and down in the liquid several times. Film evidently not thoroughly removed by this process, and therefore easily reformed	"	k 70=·0255	·43
11. Broke circuit; lifted electrodes out of liquid for 10 seconds, then moved them up and down in liquid for 30 seconds	"	k 130=·0475	·79
12. Mixed treatment	5	k 63=·0230	·77
13. Current on in α direction for a moment	"	k 67=·0245	·82

The resistance of the voltameter was, as estimated by Ohm's method, 45 ohms in the α direction of the current, and 4500 ohms in the β direction when the film was formed. Increasing the E.M.F. of the battery made no difference in the value of C.

TABLE II.—Showing Effect of (1) Change of Current-strength, (2) Previous history of Electrodes.

k = constant of galvanometer = ·00051 ampere.

The electrodes were of platinum, size not noted, and the electrolyte was strong pure H_2SO_4.

Treatment of Electrodes.	Ohms in circuit with voltameter.	Deflection.	Remarks.
1. (During experiments	30	117	Deflection steady.
2. 1–7 the current	20	162	„ less steady.
3. was kept continu-	15	191–194	Vibrations more marked.
4. ously on in the	10	235–238	
5. film-forming (β) direction.)	5	315–321	Vibrations, which were not regular.
6.	3	365–370	„ „ „
7.	0	4	Film formed; the deflection being reduced immediately to 4.
8. Short-circuited voltameter for a time.	3	4	„ „ „
9. Current reversed for an instant	3	4	„ „ „
10. Electrodes heated to redness	3	4	„ „ „
11. Short-circuited voltameter for a time.	6	4	„ „ „
12. „ „	10	212	
13. Current reversed for an instant	9	230–234	
14. „ „	7	235–250	
15. „ „	6	250–20	Film formed; deflection reduced to 20.
16. „ „	5·5	270–20	Deflection at first 270, then fell gradually to 150, and then a rapid fall to 20.
17. Electrodes reheated .	5·5	220–20	Deflection fell gradually to 20.
18. Current reversed an instant and then voltameter short-circuited	6	270–300	
19. „ „	5·5	275–310	
20. „ „	5·5	280–300	
21. Electrodes reheated..	5·5	200–4	
22. Current reversed an instant and then voltameter short-circuited	5·5	270–310	
23. Electrodes reheated and allowed to stand 5 minutes in the acid	5·5	4	Film quickly formed.

The method of placing the anode in the acid has an important influence upon the value of Δ, as the following experiments show:—

Exp. I.—The end of a Pt wire used as an anode was immersed in the acid, and then the film was formed by the lowest value of the current-density. The anode was then *gradually* lowered into the acid, when it was noticed that the film was formed over the whole wire. When the wire was lowered *rapidly* the film remained on the lower part for a time, but the upper portion was conducting, as evidenced by the escape of gas from it.

Exp. II.—The reverse of the previous experiment is not easy to perform. If we have at first the whole length of the wire immersed and use a current-density just less than Δ, and then try to form the film by diminishing the length of the wire immersed—the strength of the current meanwhile being kept constant—it is difficult to do so.

d. *Persistence of Film.*—The disappearance of the film was generally tested for by means of a weak current as follows:—Knowing what deflection would be obtained, when the film was not formed, with a certain large resistance in circuit, then after the film had been formed the resistance was increased to this value: if the current corresponding to this resistance without the film was able to pass, it showed that the film had been totally removed, and generally, from the amount of current which did pass, could be estimated the degree of disappearance of the film.

After the formation of the film there are no signs of its disappearance as long as the battery is left in circuit with the voltameter, but if the circuit be broken the film tends to disappear, the rate of disappearance depending on the completeness with which the film had previously been formed. It disappears very quickly if the voltameter be short-circuited, and instantaneously if the current be reversed.

The film is removed at once if the anode be taken out of the liquid and wiped, but if the anode be left in the liquid—the electrodes being still in connexion with the battery—the anode may be moved about in the liquid or rubbed with a glass rod without destroying the film. If the rubbing be vigorous the film will be partially destroyed, but on ceasing the friction the film immediately reforms. The anode may be removed from the liquid and held in the air some time without thoroughly destroying the film. When in the liquid a stream of air-bubbles may be blown violently against the anode without destroying the film, but if the anode is removed from the liquid a moderate current of air blown against it removes the film.

Vibrating the anode, when the film is on the point of being formed, certainly helps its formation, but after the film has formed violent agitation does not affect it, though by this means the outer layers, at least, of the sheath of bubbles round the anode may be removed. We have tried the effect of mounting the voltameter on a whirling table, the electrodes being kept fixed, and find that we can form the film when the voltameter is rapidly rotating. The value of Δ with the rotating voltameter was little, if any, different from that obtained when it was stationary. Touching the anode with an iron wire immediately destroys the film; this is also the case if a platinum wire is used to touch the anode, but when the platinum wire is not too large the film reforms.

According to our measurements the E.M.F. of polarization is about 2·5 volts; if after the film has been formed we reduce the battery E.M.F. to a value rather less than this without breaking circuit, we find that the film disappears more slowly than if the circuit is broken.

e. *Influence of Degree of Concentration of Acid.*—A small percentage of water appears to exercise but little influence on Δ, but when the liquid contains as much as 70 per cent. water by volume we have been unable to produce the film. The general effect of dilution is exhibited in the following table; the variations of Δ for percentages of acid between 98 and 87 being probably due to accidental causes. The dilution seems also to diminish the rate at which the film is formed.

TABLE III.—Showing Effect of Concentration.

A. Impure H_2SO_4. Temperature 9°·2 C.		B. Pure H_2SO_4.		
Percentage of acid by vol.	Relative values of Δ.	Percentage of acid by vol.	Temperature.	Relative values of Δ.
98	248	70	+11° C.	25
97	273	60	+13°·5 C.	62
96	272	50	+14°·3 C.	150
95	272	40*	+15°·8 C.	500
93	242	30	−10° C.	No film.
90	250			
87	250			
82	340			
78	360			

f. *Influence of Temperature.*—Inspection of the curves (figs. 6, 7, and 8), giving the variation of the film-forming current-density with the temperature will show that Δ is markedly affected by a change of temperature, and particularly so at the higher temperatures; an increase in the temperature requiring a corresponding increase in Δ, the rate of increase of Δ with temperature being greater at the higher temperatures. The observations from which the curves are drawn were obtained in the following manner:—The anode and kathode were placed in separate beakers, each containing strong pure H_2SO_4. Connexion was made between the two vessels by means of several inverted U-tubes filled with acid. The beaker containing the kathode was kept at a constant temperature throughout. Starting with both beakers at about 15° C. the current was put on in the β direction, and the current-resistance varied until the film was formed and the corresponding value of Δ noted. The current was then reversed for a moment in order to remove the film, the beaker containing the anode heated, and for every few degrees of rise of temperature Δ was found as just described. In the case of falling temperatures the process was somewhat different and yielded more regular results.

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* The production of the film in acid of this degree of dilution effectually disposes of any idea that hydroxyl can have any influence in forming the film. See footnote of our former paper, *ante*, p. 160.

After Δ had been found for the highest temperature, the resistance in circuit was slightly increased, the film being removed by reversing for an instant, and the anode vessel allowed to cool. The temperature was then noted at which the film again formed with the slightly diminished current. This procedure was repeated until the anode vessel had cooled down to the desired extent.

g. *Influence of Viscosity.*—It was thought probable that if we made dilute H_2SO_4 (say 10 per cent. acid) sufficiently viscous that the film would be produced ; but negative results have accompanied the addition of both glycerine and gelatine to the dilute acid. With a 50 per cent. acid to which 2 per cent. of gelatine had been added, the value of Δ was reduced to one third by this addition. Addition of glue to strong H_2SO_4 has a similar effect in lessening the value of Δ *.

h. *Effect of Platinizing the Electrodes.*—Some difficulty has been experienced in trying to estimate the effect which platinizing a platinum electrode has on the value of Δ, on account of the impossibility of accurately measuring the surface of the platinized wire. There is no doubt that after platinizing a few moments much greater *currents* than that which produced the film before platinization are now unable to do so, but whether the surface of the anode has increased by platinizing in the same ratio we are unable to say. In one case, in which before platinizing the film was formed with a current of 70 divisions, after a few moments' platinizing the film would not form with currents as high as 300 divisions.

3*a*. *Experiments with* H_2SO_4 *and Carbon Electrodes.*

In these experiments we at first found considerable difficulty owing to the disintegration, during electrolysis, of the various kinds of carbon. Carré-prepared carbons, gas-carbons, and graphite (from lead-pencils) all behaved in the same way. On making circuit no gas at first comes from the anode, but eventually the outer shell breaks off and gas is evolved*. It occurred to us that the coherent carbon filaments used in incandescent lamps might be free from this objection, and this conjecture was found to be justified. At first filaments from Swan lamps were used, and afterwards some kindly supplied by Messrs. Woodhouse and Rawson. With either of these carbons we were able to electrolyze strong H_2SO_4 without any disintegration or coloration of the liquid.

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* It is perhaps worth mentioning that a jelly made of glue and water gave the film at the kathode. In one case, when the electrodes were a wire and a Wollaston's point, the resistance was 2000 ohms when the wire was kathode, and 45,000 ohms with the point as kathode.

Using these carbon filaments we were able to readily produce a film at the anode which had the same general properties as that obtained with platinum. To make sure that the film was not due to any coating of oxidizable material the filaments were boiled in aqua-regia and well washed, but this had no influence in preventing the formation of the film. Platinized carbon behaves as regards the film-formation exactly like platinized platinum. The insulating condition, with carbon filaments, is accompanied, as in the case of platinum, by the formation of a gaseous sheath round the anode.

3 *b*. *Experiments with* H_2SO_4 *and Gold Electrodes.*

Some fine gold wire, sold to us as pure by Messrs. Johnson, Matthey, & Co., and a piece of platinum foil were used together as electrodes in strong pure H_2SO_4; the platinum and gold being placed in separate limbs of a U-tube. The behaviour of this combination was very peculiar. The film readily formed when the gold was anode, but no sooner had it formed than it began to disappear again, and did not reform until the circuit had been broken. On making the gold wire the anode, after the current had been in the previous direction, it was noticed that one of two things happened, either (1) the deflection at first was large, and decreased rather quickly to a small value, then increased gradually to its former value, or (2) the deflection at first was small and then made a sudden jump to a high value, at which it remained. The gold wire used was very rapidly eaten away.

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* This behaviour of carbon has been noticed by Gore and others, and more recently studied by Debray and Pechard (*Compt. Rend.* cv. pp. 27–30). These observers, using gas and other kinds of carbon purified by Cl at high temperatures, found that the gas evolved at the anode during the electrolysis of H_2SO_4 was a mixture of CO_2 and O. The black powder formed on disintegration (after being washed and dried) deflagrated at a temperature below red-heat, evolving CO and CO_2.

4. *Experiments with other Electrolytes and Platinum Electrodes.*

a. *Phosphoric Acid.*—Efforts to produce a definite film have been unavailing. We have tried various solutions (including some quite viscous), and although we have found that there is a difference of deflection in the two directions of the current, the electrodes being a point and a plate, we are disposed to attribute this mainly to polarization. At the same time there certainly is evidence of a difference of transition resistance in the two directions, for in one case, using a battery E.M.F. of about 18 volts, the deflection when the point was anode was double that with the current reversed. This indicates that in the latter direction there is a higher transition resistance, for any difference of polarization in the two directions would probably be small relative to the E.M.F. employed.

b. *Caustic Potash.*—Different strengths of solution up to saturation, and a battery E.M.F. up to ten volts were used, but no definite film could be obtained. On adding glycerine to the solution, in order to increase its viscosity, the transition resistance, as measured by Ohm's method, was greater with the point anode than when kathode; in one experiment the resistances were 37 and 3·5 ohms respectively. Attempts were afterwards made to repeat the effects obtained by Ohm, with solutions of caustic potash, and for this purpose a battery of 3 Daniells was employed. The differences of the deflections in the two directions, and also the fall of the deflection obtained after making circuit, were generally to be attributed to polarization; but by using certain currents a tendency towards the formation of a transition resistance could be traced, as evidenced by the table below, the numbers of which were obtained by Ohm's method.

TABLE IV.—Irreciprocity with Platinum Electrodes in KHO *.

	Point anode.	Plate anode.
Resistance of voltameter, Exp. 1...	111·3 ohms.	33·5 ohms.
" " " 2...	120 "	32 "
Polarization " " 1...	2·65 volts.	2·12 volts.
" " " 2...	2·65 "	2·16 "
Current through " " 1...	·00265 ampere.	·0178 ampere.
" " " " 2...	·00247 "	·0149 "

c. *Other Liquids.*—No film was obtained with platinum electrodes in the following liquids :—(1) Pure fuming HNO_3; (2) a mixture of HNO_3 and H_2SO_4, the former being in excess ; (3) strong HCl ; (4) a mixture of HNO_3 and HCl in equal parts. The film was obtained with strong H_2SO_4 in which CrO_3 had been dissolved, but no remarkable features were noticed.

5. *Experiments on Soap and Sodium Benzoate.*

A piece of common white soap was cut in the form of a cylinder 4 cm. long and 1 cm. diameter. It was protected by a glass tube, and mounted so that platinum plates pressed against its two ends. The resistance, as measured by Ohm's method, showed a gradually increasing value, the battery being kept, in circuit, from 1080 to 23,000 ohms, at which it remained constant. The polarization was taken for various times of charge, but showed but little variation. Measurements of the resistance of the soap with a battery in its circuit were also made by Kohlrausch's method, the results confirming those previously obtained by Ohm's method. With solutions of soap the solid soap acids were separated out, but there were no signs of any film-formation.

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[*] Further measurements are being made both with KHO and other liquids by means of the method of Fuchs, which with certain precautions now appears to us to be the one best adapted.

In many respects a solution of sodium benzoate resembles soap, for benzoic acid is but little soluble in water, hence it was thought that an electrolytic deposit at the anode might stop the current. This was found to be the case, different strengths of sodium benzoate giving the film without difficulty. Careful observation showed that with weak currents the oxygen produced at the anode was able to break through the layer of benzoic acid, and thus prevent the film-formation; but with strong currents the solid acid obtained the mastery, the oxygen then not being able to break through and so disperse the acid. Hence, with strong currents a film, consisting of solid benzoic acid in which oxygen bubbles were entangled, was formed round the anode, thus stopping the current.

6. *Discussion of the possible Causes of the Film.*

The film in H_2SO_4 with platinum electrodes is in many respects very analogous to those obtained in the same liquid with oxidizable electrodes, and also to that which causes the *passivity* of iron, in which cases a solid oxide or sulphate layer on the anode is acknowledged to be the cause of the insulation. The non-formation of the film with platinum electrodes in H_2SO_4 below a certain strength, the increase of Δ with the temperature, the fact that the film disappears gradually on breaking the circuit, and immediately on reversing the current, all point, it may be argued, to a solid oxide or sulphate layer on the platinum anode; the solubility of this layer varying with the concentration and temperature of the acid, and its disappearance being immediately caused by a deposit of nascent H on it. For although platinum is usually regarded as non-oxidizable, yet under the condition of electrolysis, which is attended by the production of ozone in quantity at the anode, it seems quite possible for an oxide of platinum to be formed. This view was strongly upheld by De La Rive. In his 'Electricity,' vol. ii. p. 410, he says that "the part played by platinum is very remarkable, for although it passes for being not oxidizable, yet it comports itself like the metals that are so, but in a feeble degree." The blackening of platinum electrodes in dilute H_2SO_4 caused by the passage of alternate or direct intermittent currents (the former being more effective) is urged as a proof that platinum is attackable by the electrolytic gases. De La Rive insists upon the view that the blackening is due to a series of oxidations and reductions of the platinum, the oxidation with the direct currents being due to the O dissolved in the acid. Another view, mentioned by Wiedemann, is that the alteration of the surface is caused by the mechanical action of the hydrogen, which is first occluded and then evolved again from the electrode when the current is diminished or reversed. De La Rive gives other evidence in support of his theory of the successive oxidation and reduction of platinum, and explains the catalytic action of platinum according to this view. Schönbein, on the other hand, quite as firmly upheld the theory that the oxygen did not combine with the platinum, but formed a condensed gaseous film on its surface. Faraday also seems to have shared this view*.

Again, it should be mentioned that, using large current-densities, McLeod † found that very fine platinum wires were destroyed during the electrolysis of H_2SO_4 of density 1·3; but whether this was due to actual solution of the platinum or to its disaggregation or to traces of Cl is doubtful. If solution of the platinum really occurs, it would support the theory that the film is due to a solid oxide or sulphate layer, as in the case of the oxidizable electrodes. In this relation an experiment of ours may be quoted. A platinum wire was carefully cleaned and weighed, and then used as anode in strong H_2SO_4, the film being then formed on it. It was taken out quickly, and wiped (with asbestos), which, as said previously, removes the film. The anode was then replaced, the film again formed and again wiped off. This was repeated a great number of times, but no variation of the weight of the anode could be detected after the experiment, nor was there any difference in the appearance of the wire. We have further electrolyzed sulphuric acid with platinum electrodes continuously for many hours without finding any variation in the weight of the electrodes. Negative experiments of this nature, however, cannot be regarded as having much value.

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* Exp. Res. vol. i. p. 165. It may be noted that Dulong and Thenard have shown in a research (*Ann. de Chim.* vol. xxiii. p. 440, 1823, and vol. xxiv. p. 380, 1823) that all bodies have the catalytic power, more or less.

† Journ. Chem. Soc. vol. xlix. p. 591.

In endeavouring to ascertain the nature of the film we are met by the difficulty that removal from the acid speedily brings about the destruction of the film, and other evidences of its instability have been previously given. Thus, there is no possibility of applying the direct tests that have led to the generally accepted view in the case of the oxidizable electrodes, and although the film sticks with some firmness when in the electrolyte and the battery-current is not broken, yet there are no chemical tests which can be directly applied under these conditions that can be thought very satisfactory. We may, however, mention that the film remains undisturbed when a stream of hydrogen is passed in small bubbles over the anode, but is speedily removed if the acid round the anode be diluted by a stream of water. The formation of the film with carbon electrodes, the non-formation of the film with platinum electrodes in various oxidizing agents, and other minor properties, which will be found in the previous part of the paper, have led us to doubt the accuracy of the oxide theory in the case of platinum or carbon electrodes, and to *provisionally* adopt another theory, as indicated below.

The behaviour observed at the anode in the case of the electrolysis of sodium benzoate solution suggests that the badly-conducting sulphuric acid which appears, in electrolyzing that liquid, at the anode, may under certain conditions be able to stop the current. In discussing such a hypothesis we have to remember :—

(1) That the specific resistance of H_2SO_4 increases very rapidly with the degree of concentration.

(2) That the concentration of the acid at the anode during electrolysis is partially prevented (*a*) by the evolution of oxygen (as we have pointed out, *ante*, p. 164), and also (*b*) by its dilution, the rapidity of which depends on the viscosity and strength of the acid solution used.

(3) That a certain amount of oxygen forms a gaseous layer on the anode.

Bearing these points in mind, we will examine whether they are sufficient to explain our experiments.

Firstly. With low current-densities the concentrated acid will be formed in quantity too small to withstand the two causes tending to cause its dispersion.

Secondly. With greater current-densities the rate of formation of the concentrated acid will overcome the rapidity of its dilution, and the resistance of the layer of acid round the anode may increase so rapidly as to form an insulating film, before enough gas has been evolved to cause its destruction; the gas which has been produced becoming entangled in the viscous concentrated layer, which, once formed, will not be easily removed by friction, &c.

Thirdly. With still higher current-densities, though there will be a greater tendency towards the formation of this layer immediately the circuit is made, yet it is conceivable that more gas will be produced in the same time than can be prevented from escaping, and thus destroying the non-conducting layer; for it would be expected that the tendency of the gas to escape would increase at a greater rate than the tendency of the acid layer to prevent the escape. An *upper limit* to the current-density ought therefore to be discoverable beyond which no film can be produced. In the case of foil electrodes placed horizontally with only the lower surface exposed to the acid, we have succeeded in obtaining such an upper limit. The discovery of an upper limit makes some of our experiments of less value, for where we have stated that no film has been obtained, the non-success may be due to failure in fixing on the right limits of current-density. It also explains the peculiar jumps in the current which we have at times observed in using platinum electrodes in strong H_2SO_4. These jumps, which were of a similar kind to those which we obtained with gold electrodes, are probably due to the current-density used being near the upper limit when small causes, such as the heating of the liquid, would involve the disappearance of the film*.

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* We are not disposed to think that the jumps obtained with gold are due to the same cause. Gold really comes under the head of attackable electrodes, and the phenomena with it are, therefore, probably of a more complicated character than with either platinum or carbon. Palladium, in this respect, must be classed with gold, for, when used as anode in moderately strong sulphuric acid, it dissolves.

This provisional hypothesis likewise accounts for the non-formation of the film in dilute H_2SO_4*. Here the rate of dilution (even if the electrolyte be made viscous by the addition of glycerine) of the concentrating layer will be more rapid, and hence a greater length of time would be required to produce a thoroughly concentrated layer. But the longer the time that elapses before this is attained, so much the greater must be the quantity of oxygen produced. Thus, just as in the case of current-densities above the upper limit in strong H_2SO_4, the formation of film will be prevented.

The less rapid increase of the specific resistance with the degree of concentration in the cases of H_3PO_4 and KHO probably explains the difficulty found in obtaining such a high degree of insulation as we have observed with H_2SO_4.

Some other aspects of transition resistance, evidenced by some experiments not included here, will be considered in a subsequent paper.

XLVI. *Note on some Additions to the Kew Magnetometer.* By T. E. Thorpe, *Ph.D., F.R.S., and* A. W. Rücker, *M.A., F.R.S.*†

In making field observations with the Kew magnetometer it is important that the mirror by which the image of the sun is formed should either be in perfect adjustment, or that the errors should be known and allowed for.

The axis about which it rotates is made horizontal by means of a riding level. The plane of the mirror is made parallel to the axis by adjusting it until the image of the cross wires formed by reflexion in the mirror does not alter its position when the mirror is inverted in its bearings. The axis of the mirror is made perpendicular to the optic axis of the telescope by making the cross wires and their image coincident. In the course of the Magnetic Survey of Great Britain and Ireland, on which we have been for some time engaged, we have found such great difficulty in making these adjustments in the field, that we have for a long time practically abandoned this method, and adjusted the mirror from time to time, either indoors by means of a plumb-line, or by observations on some convenient object external to the instrument in the field. We have always, when possible, made four sets of sun observations, reversing the mirror in its bearings, and taking "front" and "back" views, in which the observer looks towards the sun and stands with his back to it respectively. We have thus been able to eliminate the errors of adjustment of the mirror, and to assure ourselves that each observation was satisfactory.

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* The results obtained by Christiani, we believe, may also be explained by this hypothesis.

† Read June 9, 1888.

This method is, however, open to some objection. When the sky is cloudy it may be impossible to see the sun for a time sufficient to take all four sets of observations. On such occasions we always pay special attention to the adjustment of the mirror, but it may happen that no object near the station is sufficiently elevated to be used for the purposes of correction. In that case it is necessary to rely on the result of a long experience, which proves that if reasonable care is taken of the instrument the mirror does not get seriously out of adjustment during a journey by road or rail extending over a day or two.

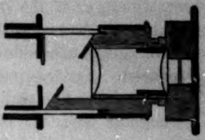
Nevertheless, there can be no doubt that the method of correcting by the image of the cross wires has the advantage that it can, theoretically at all events, be carried out at the place and time at which the observations are made.

The practical drawback which led us to abandon it was the difficulty of seeing the image of the wires. In the neighbourhood of the cross wires the metal of the telescope tube is cut away, but the light thus admitted is insufficient. On a gloomy day, when the sun was only visible at rare intervals, and when, therefore, it was most important that the mirror should be in order, the necessary adjustments could not be made. Even on a bright day it was desirable to envelop the observer's head and the instrument in a dark cloth, and this was difficult and troublesome if the wind was high.

We have, therefore, lately devised some additions to the magnetometer which, although extremely simple, make it practically possible to use the cross wires for making the adjustments.

A polished platinum mirror is introduced into the telescope between the eye-piece and the cross wires, which are viewed

Fig. 1.



through a hole in its centre. The mirror is inclined at 45° to the axis of the telescope, and reflects a considerable quantity of light upon the wires. A bronzed brass tube is attached to a support which fits into the three holes which are provided to fix the magnet-box in position. This tube extends from the telescope to the mirror, and thus shuts out extraneous light. It can be packed in the ordinary magnetometer-box, and when it is in position, and the platinum mirror is directed towards the sky, or, if desirable, towards its brightest part, the image of the wires can readily be seen. If the background reflects much light, it is desirable to place a small blackened metal screen weighted with lead on the horizontal circle which carries the mirror. This can be lifted off without in any way interfering with the adjustments.

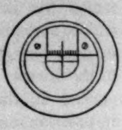
The following two examples may serve to indicate the degree of precision with which the adjustment of the mirror can be made.

Observations were taken both when the observer was looking towards and directly away from the sun. The N point on the circle is deduced from the front observations. The collimation correction is obtained by combining the front and back observations. It will be seen that the uncorrected results are quite as good as those to which the collimation correction has been applied, which proves that the mirror can be adjusted to within the limits of the error of observation on the sun.

Station.	G. M. T.	Altitude of Sun.	N point on Circle.	Collimation correction.	Corrected N point.
	h. m. s.	° ′	° ′	°	° ′
Horsham. (April 21, 1888.)	12 43 49	50 2·2	297 4·2	+0·3	297 4·5
	15 15 44	34 33·7	297 3·8	−0·6	297 3·2
	17 4 0	18 12·7	297 3·6	−0·1	297 3·5
Alresford. (April 28, 1888.)	14 10 36	45 7·8	241 23·0	−0·1	241 22·9
	15 45 6	....	241 22·5	Not determined.	

In cases where it is for any reason impossible to take the back observations, or if the sun is so frequently covered that it is advisable to observe without waiting to adjust the mirror, it is advantageous to be able to determine how much error has been introduced. For this purpose we have placed in the plane of the cross wires a fine scale engraved on ivory. The angular deviation of the mirror which produces a given displacement of the image of the cross wires on this scale can be determined, and when this is known the error of the mirror can, on any subsequent occasion, be deduced from the deviation of the image.

Fig. 2.



The following observations were made in a lofty room on an object the elevation of which was 40° :—

Scale Reading of Image of Cross Wires (x).	Reading on Circle (ρ).	$x-x_0$.	$\rho-\rho_0$.
−5·65	264° 42′·1	−5·45	6·4
−3·60	264 39·5	−3·40	3·8
−0·20	264 35·7		
+2·80	264 32·6	3·00	−3·1
+5·45	264 29·0	5·65	−6·7

All the readings are the mean of two which differed but little, and between which the axle of the mirror was inverted in its bearings. Hence if α be the angular value of a scale-division, *i. e.* the error of collimation of the mirror which corresponds to a displacement of the image through one division,

$$\rho-\rho_0=\frac{2(x-x_0)\,\alpha\sin^2 20^\circ}{\cos 40^\circ}$$

$$=0{\cdot}306\,(x-x_0)\,\alpha.$$

We thus get four values of α, viz. 3′·8, 3′·7, 3′·4, and 3′·9, the mean of which, viz. 3′·7, may be taken as correct.

To check this value, observations were made at South Kensington on the sun, and on a pinnacle of the Natural

History Museum, both when the mirror was in good and when it was in bad adjustment. It will be seen that the corrected values are in close accord.

Thus on May 9th, at $12^h\,46^m\,36^s$, when the altitude of the sun was $54^\circ\,39'{\cdot}8$, the N point on the scale was found to be $58^\circ\,11'{\cdot}2$. The mirror was then thrown out of adjustment, the mean reading for the image of the cross wires being $+5{\cdot}7$ div. Another observation on the sun was made at $1^h\,13^m\,12^s$, when the altitude was $52^\circ\,51'{\cdot}5$, which gave for the reading of the north point $57^\circ\,56'{\cdot}9$.

The correction is

$$\frac{2\times 5{\cdot}7\times 3{\cdot}7\,\sin^2 26^\circ 26'}{\cos 52^\circ\,51'}=13'{\cdot}8.$$

Hence

$$57^\circ\,56'{\cdot}9+13'{\cdot}8=58^\circ\,10'{\cdot}7,$$

which differs only by half a minute from the value obtained when the mirror was in adjustment.

In like manner the bearing of a pinnacle, as given by the mirror in adjustment, was $128^\circ\,40'{\cdot}8$, while, when the reading for the image of the cross wires was $+5{\cdot}7$ div., it was.

$$128^\circ\,40'{\cdot}2+0'{\cdot}8=128^\circ\,41'{\cdot}0.$$

The elevation was $15^\circ\,39'$, and the two results agree to $0'{\cdot}2$. It is evident from these test cases that, since the error of the mirror was far larger than that produced by travelling for a day or two, observations in the field can, by means of the scale, be corrected to within the limits of the error of experiment.

The additions to the magnetometer, which we have here described, were made for us by Messrs. Elliott, and we venture to think that they might with advantage be incorporated in standard instruments of the Kew pattern.

XLVII. *Note on Continuous-current Transformers.*

By Professor SILVANUS P. THOMPSON*.

IT has often been proposed to distribute electric energy from central stations to local distributing stations by means of transformers, which receive small currents at high potential and transform them into large currents at low potential. When alternating currents are employed, induction-coils of appropriate construction are used as transformers. But the use of such currents is attended by two disadvantages: namely, that alternate currents cannot be used for electrochemical purposes, and that, pending the invention of a satisfactory alternate-current motor, they cannot be used for the actuating motive machinery. When continuous currents are used, the appropriate transformer may be one of two types —(1) the motor-generator, (2) the commuting-transformer.

The motor-generator in its primitive form consists of a motor to receive the incoming current, geared mechanically to a generator which produces the outgoing current. A more specialized form consists of a single machine with one field-magnet and two armatures; one to receive the primary current, the other to generate the out-going or secondary current. If the two sets of armature-windings are coiled around the same core, each set of windings being furnished with an appropriate commutator and collecting-brushes, the development has reached its extreme case, the only remaining point being the proper method of excitation of the field-magnet.

The commuting-transformer is a more complex apparatus, and has been much misunderstood. It does not necessarily involve any greater amount of sparking than any ordinary dynamo. The general principle of this species of transformer may be explained by reference to a particular type. Suppose that an armature double-wound with a primary coil to receive the incoming current, and a secondary coil to generate the outgoing current, have been provided as in the previous case, each coil having its appropriate commutator. To enable this piece of apparatus to transform the currents, it must either be allowed to rotate in a magnetic field between the poles of a field-magnet, or else the field-magnet (and collecting-brushes) must be arranged to rotate, in the opposite sense, around it. But there is a third possible arrangement, namely to rotate around it the magnetic polarity while itself remains fixed. This may be done either by rotating the brushes which bring in the primary current, the magnetic circuit being completed by a mass of iron external to the ring, or by rotating also the polarity of a fixed field-magnet constructed specially for this purpose. An external fixed ring of Gramme or Pacinotti pattern, provided with a commutator, will answer for this purpose, the current being supplied by a pair of brushes which is rotated. In such a machine obviously all the parts are stationary save the revolving brushes, which must be driven by some mechanical device. Various modifications of this idea have from time to time been suggested.

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* Read June 23, 1888.

The theory of alternate current-transformers has been investigated by various authorities. It was first shown by Maxwell that where there is mutual induction between two circuits, the effect upon the secondary circuit of the presence of the primary circuit is threefold :—(*a*) to transform in a certain ratio the electromotive force ; (*b*) to add to the resistance of the secondary circuit an apparent resistance equal to that of the primary multiplied by the square of the same ratio; (*c*) to deduct from the coefficient of self-induction of the secondary circuit a quantity equal to the coefficient of self-induction of the primary circuit multiplied by the square of the same ratio. The ratio in question was found to be the quantity

$$\frac{2\pi n M}{\sqrt{4\pi^2 n^2 L_1^2 + R_1^2}};$$

where M is the coefficient of mutual induction, n the number of alternations per second, L_1 and R_1 the coefficient of self-induction and the resistance respectively of the primary circuit. In a communication made last year to this Society I showed how, assuming the proper conditions of good construction to have been observed, this ratio was equal to the ratio of the number of secondary windings to the number of primary windings in the transformer ; which ratio is known as the "coefficient of transformation."

The object of the present paper is to show that in continuous current-transformers effects of the same kind exist. The investigation is of an elementary character, secondary reactions being assumed to be negligibly small.

Consider a motor-generator, with double-wound armature of ring or drum type, arranged to rotate between the poles of a common fixed field-magnet. The magnetism of the latter may be considered for present purpose constant: the effect of variation in the magnetization will be afterwards considered. Let the number of armature-conductors of the primary or motor part, as counted all round the periphery, be called C_1, and that of the secondary part be called C_2. The ratio of C_2 to C_1 we may call *the coefficient of transformation*, and we shall use k as the symbol of this ratio.

Now let i_1, r_1, and E_1 stand respectively for the current, the resistance, and the induced electromotive force in the coils of the primary armature, and i_2, r_2, E_2 for the corresponding quantities for the coils of the secondary armature. Write N for the whole number of magnetic lines passing through the armature-core. Also write e_1 and e_2 for the respective differences of potential at the terminals of the primary and secondary parts.

We shall then have

$$E_1 = nC_1N \,.\, 10^{-8},$$

$$E_2 = nC_2N \,.\, 10^{-8};$$

whence

$$E_2/E_1 = C_2/C_1 = k,$$

whatever the values of speed and field may be. Further,

$$E_1 = e_1 - r_1 i_1,$$

$$E_2 = e_2 + r_2 i_2;$$

whence

$$e_2 = ke_1 - r_2 i_2 - kr_1 i_1.$$

Now assume that the work wasted in the armature in mechanical and magnetic friction, and in producing eddy-currents, is negligibly small compared with the work done in driving the generator part. This is, in fact, merely assuming that the machine is properly designed and constructed; for in such machines, there being no driving-belt, the only forces except gravity are centrally balanced, and mechanical friction at the bearings is a minimum; and further, if the iron core is of proper quality and quantity, and properly laminated and insulated, the losses due to hysteresis and eddy-currents are very small*. We shall then have the work $E_1 i_1$ done by the primary current (in unit time) equal to the work $E_2 i_2$ done on the secondary current. Consequently $i_1 = k i_2$. Inserting this value, we at once get

$$e_2 = k e_1 - (r_2 + k^2 r_1) i_2.$$

This shows that everything goes on in the secondary circuit as though the induced electromotive force was transformed from the difference of potentials at the terminals of the primary circuit in proportion to the respective numbers of windings on the armature, and as though there were then added to the real internal resistance of the secondary circuit a resistance equal to that of the primary winding multiplied by the square of the coefficient of transformation. If there are equal weights of copper in the primary and secondary windings, the actual resistances of the two windings will be proportional to the squares of the numbers of turns: hence the effect of multiplying r_1 by k^2 is to make it equal to r_2; in other words, the added virtual resistance is equal to the real internal resistance.

With respect to self-induction and its effects in such machines, it may be remembered that, in an ordinary single-wound dynamo, the effect of self-induction is to add to the armature a spurious resistance, owing to the successive stoppage and restarting twice in every revolution of the current in each section. Now if matters were so arranged in the double-wound machine that commutation of the current in any one section of the primary should occur exactly at the same instant as commutation of the current in that section of the secondary wire which was wound over the same part of the common core, it is clear that, as the currents in the primary and secondary circulate around the core in opposite senses, the stopping of the current in the one would tend to stop the (inverse) current in the other, and the starting again of the current in the one would tend to start the inverse

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* If F be the waste work done against mechanical and magnetic friction, the formula becomes

$$e_2 = k e_1 - i_2 (r_2 + k^2 r_1 + F/i_2^2).$$

current in the other. In other words, the mutual induction between the two sections would tend to counteract in both the effects of self-induction. In practice it is impossible to fully realize this neutralizing effect; but that it is very nearly realized is evidenced by the almost complete absence of sparking in such machines.

In the above argument it has been assumed that the magnetism of the field-magnet was constant in amount; but incidentally it was noticed that the expressions were independent of the magnetic field. The more powerful this is, the slower need the armature run to generate the respective electromotive forces. Assuming that the transformer is supplied at constant potential at its primary terminals, and that the internal resistance of the secondary winding is small, it will be self-regulating, giving a constant potential at its secondary terminals quite irrespective of the variations of its speed with the load.

In yet one other respect does the action of the motor-generator resemble the alternating transformer: when supplied at constant potential it is almost exactly self-regulating in respect of its automatic action in adjusting the amount of the inflowing primary current, in proportion to the outflowing secondary current. When the secondary circuit is entirely opened, the motor part runs just so fast that the back electromotive force E_1 in the primary part increases and dams back the primary current; only just so much flowing through as will suffice to drive the machine against the reactive forces of mechanical and magnetic friction.

Precisely similar relations to those traced out for motor-generators hold good in the commuting-transformers. The reactions between the primary and secondary windings go on exactly the same, whether the successive displacement in the polarity of the magnetization through the common core of the armature be accomplished by mechanically rotating it, or by electrically shifting the polarity of the surrounding field-magnet in a rotatory fashion. With equal weights of copper in the primary and secondary coils of the armature part, the effect of mutual induction will here also be approximately to double the internal resistance and to neutralize the self-induction of the secondary winding.

XLVIII. *On the Existence of an Undulatory Movement accompanying the Electric Spark.* *By* ERNEST H. COOK, *D.Sc.* (*Lond.*), *F.I.C.*, *Merchant Venturers' School, Bristol*[*].

[Plate XI.]

1. IF a powdered substance be scattered over a smooth plate, and the plate held near a pair of terminals between which an electric spark is passing, the powder will arrange itself in a more or less regular series of concentric circles. The common centre of these circles (when the spark is small) appears to be a point directly under that halfway between the terminals. The general appearance presented by these curves will be apparent by a glance at the figures in the Plate which accompanies this paper. These are approximately of the same size as the original. It will be noticed that the figures roughly divide themselves into two classes—(1) those with a clear space in the middle, and (2) those with a circular space, within which the powder is apparently unaffected.

The first class are obtained by holding the plate near to the terminals, the distance varying with the energy of the spark. The cleared space is irregularly elliptical, with its major axis at right angles to the direction of the spark, and the area of the space increases as the intensity of the spark increases. The second class are formed when the distance between the spark and plate is increased until the elliptical space ceases to make its appearance.

2. *Mode of Obtaining the Curves.*—The most convenient arrangement and the one which gives the best results is as follows:—An induction-coil has each of its terminals connected with a Leyden jar, and with one of the handles of a universal discharger. The plate with the powder scattered over it from a sieve is held under the points at a distance which can be varied at pleasure. The distance between the terminals, and therefore the length of the spark, can also be easily adjusted. On connecting the coil to the battery a

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[*] Read June 23, 1888.

The paper was illustrated by a series of photographs: from two of these the figures in the Plate have been engraved.

succession of sparks of condensed electricity is obtained, and the powders will be seen to arrange themselves in the manner described. The first spark causes a distinct effect, but a continuance is necessary to bring out the regularity of the curves.

Should it be required to fix the powders in the positions which they have taken up, it is easily done by coating the plate beforehand with a thin layer of gum, and after the experiment breathing on it.

3. *Effect of Plate.*—When the figures were first obtained it was surmised that they might be caused by the distribution of the electricity over the plate; that they were in fact modified Lichtenberg's figures. Experiments were therefore made with plates of various substances in different states as regards their surfaces; for example, whether polished or dull, electrified or unelectrified. The following materials have been examined:—glass; resinous cake of electrophorus (a mixture of shell-lac, resin, and Venice turpentine); ebonite; paper (glazed and unglazed); brass (lacquered and unlacquered); zinc; iron, tinned ditto, galvanized ditto; wood; paraffin; cardboard; and glazed and unglazed earthenware. Except that the rougher surfaces retard and prevent the regularity of the curves, no difference whatever could be observed as due to the substance or condition of the surface.

A piece of plate-glass or polished sheet of metal answers admirably for these experiments. The fact that the production of the curves is independent of the electrical state of the support (and therefore of the powder) justifies the statement that they are not caused by any peculiarity of electrical distribution.

4. *Effect of Battery Power, Coil, and Condensation.*—Experiments were made with the view of finding if variations in the number of cells, in the coil, and in the capacity of the condensers used, caused any alteration in the shape or frequency of the lines in which the powders arranged themselves. Other conditions remaining constant, the battery power employed to drive the coil was changed from ten pint bichromates to one. In no case could any difference be observed in the resulting figures produced. Afterwards the current was still further reduced until it was just sufficient

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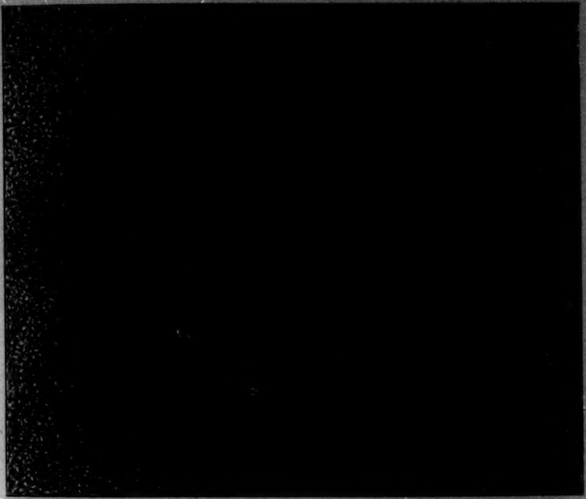

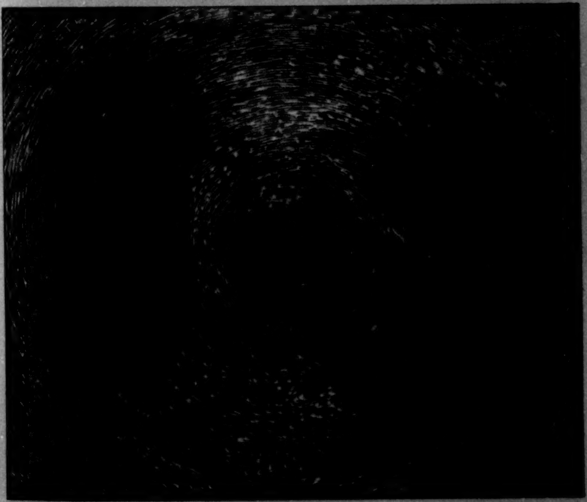

Mintern Bros. lith.

to drive the coil, but so long as this was accomplished the figures remained unaffected.

Several coils were used in the course of the earlier experiments, but finally the work was confined to two. One of these was a very fine instrument by Ruhmkorff, kindly lent to me for the purpose of these experiments by Mr. Francis J. Fry, late High Sheriff of Bristol. It contains a secondary coil, formed of twenty-four miles of wire, is fitted with both spring and mercury interrupters, and with one bichromate (carbons 7″ × 3½″), gives a regular succession of sparks six inches long. The second was a smaller instrument, by Paterson of London, fitted with a condenser of 100 sheets of 12″ by 5″, and capable of giving a small spark of about 1½ inch long. When coupled with the Leyden jars, of course, the length of spark was much diminished. In no case with either coil was a longer spark employed than ·75 inch. No change in the appearance, regularity, or shape of the figures was caused by replacing one coil by another.

The effect of a difference in condensing-power was tried by interposing more jars. Except that the vigour of the spark caused a larger area of cleared space, no alteration could be observed. This is, of course, confirmed by the fact of the different coils, possessing condensers of different areas, giving identical results.

5. *Influence of Contact-Breaker.*—It might be supposed, as was done by Grove in reference to the stratifications in partial vacuo, that the intermittent character of the currents flowing in the secondary wire caused by the interruptions in the primary-coil by the contact-breaker would have some influence in producing or regulating the curves. This, however, is not the case. The shapes did not alter no matter what coil was employed, and when the mercury-break on the large coil was used in place of the spring, no alteration in the resulting curves was produced. Moreover the curves can be obtained without the use of a coil with its necessary contact-breaker at all. For this purpose it is only necessary to connect each electrode of a Wimshurst machine to one or two Leyden jars, and to allow the highly condensed spark thus obtained to act upon the powder. In fact I have been able to produce the curves in this way without employing any jars at all. With

the machine, however, they are far inferior in regularity to those produced by the use of the coil. With a 17-inch plate the best results have been obtained with jars having a total area of foil of about 1500 square inches, a spark of about 3″ long, and the powder 6″ below spark.

6. *Nature of Electrodes.*—Varying the material forming the points between which the sparks were taken made no difference in the resulting shapes. Thus the same effects are produced by using brass, iron, or carbon points.

7. *Influence of the Powdered Substance.*—Soon after the first results were obtained and upon trying different substances, it became evident that, although accidental irregularities occurred, yet, upon the whole, the curves were remarkably regular. It was also very soon noticed that the frequency with which they followed each other altered with an alteration of the substance of which they were composed. This induced a desire to compare by measurement the distances between each successive curve. A large number of different powders were examined, and the number of lines in a certain distance counted. Several determinations were made and the mean number taken. Although every care was taken to make these measurements as accurately as possible and to avoid sources of error, yet, from the difficulties peculiar to the case, I do not wish to pledge the absolute accuracy of the numbers, but put them forward as approximate only. It first became necessary to ascertain if the fineness of the powder affected the number of lines in a certain length. This was found not to be the case. Provided the degree of fineness was such as to admit of the production of the lines with such an amount of definiteness as sufficed for their measurement, the number of lines was found to be unaltered. The following tabular statement gives the results obtained. The actual experiment was to count the number in a quarter of an inch. These have been multiplied by 4, hence the regularity of the diminution of the numbers given in the first column:—

No. of lines in 1 inch.	Substances examined.
88.	Silica (powdered sand).
80.	Magnesia alba.
68.	Chromic oxide.
64.	Antimony sulphide; baric peroxide; fluor spar; cupric oxide; red lead; lead acetate; lead oxide; antimony; sulphur; potassium hydrogen tartrate.
60.	Borax; cobaltic oxide; starch; zinc carbonate.
56.	Binoxide of manganese; baric carbonate; tricalcic phosphate; ferrous sulphide; lead chromate; mercuric oxide; binoxide of tin; nickel monoxide; lead carbonate; calcic oxide.
52.	Sodium hydrogen carbonate; potassic sulphate; potassic carbonate; mercurous chloride; carbon; calcic hydrate.
48.	Tannin; salicylic acid; rochelle salt; ammonic sulphate; baric nitrate; copper sulphate; oxalic acid; sodic chloride; succinic acid; Epsom salts.
44.	Alum.
40.	Chalk.
The figures were so indefinite as not to permit counting.	Iron filings; baric chloride; baric sulphate; benzoic acid; strontium chloride; bismuth nitrate; cadmium nitrate; microcosmic salt; potassium chloride; potassium bromide; ammonium chloride; ferrous sulphate; ammonium nitrate; pyrogallol.

8. *Effect of Mixture.*—While engaged in repeating the experiments, in order to confirm the numbers, it was found that a particular sample of calcic oxide gave 48 lines to the inch instead of 56, as given in the table. Upon examining the sample it was found, however, to be an old one, and to be partially converted into carbonate. It was thus a mixture of oxide and carbonate. This led to the investigation of other mixtures. It was thus proved that the number given by a mixture of two substances was intermediate between those of its constituents. The following were examined, no particular care being taken to mix in definite proportions, but the substances simply ground up together in a mortar:—

Mixture.	Nos. due to each constituent.	No. obtained.		
		Exp. 1.	2.	3.
Calcic oxide Calcic carbonate	56 40	48	50	48
Magnesia alba Calcic carbonate	80 40	60	64	56
Cobaltic oxide Chromic oxide	60 68	64	64	66
Cobaltic oxide Magnesia alba	60 80	68	64	68

9. *Effect on Liquids.*—In order to test if the effect could be produced on the surfaces of liquids the following experiments were made:—In place of a glass plate with the powder scattered over it, there was held under the spark a shallow vessel (an ordinary porcelain evaporating dish answers very well) containing the liquid. So long as the surface was looked directly down upon, no effect could be observed; but when regarded obliquely, minute ripples were seen to be produced upon the surface. No attempt was made to measure these, but they were judged to be of about the same size as those of the powders, that is, between the limits of 88 and 40 to the inch. The following were the liquids tested in this way:—water, mercury, alcohol, ether, glycerin, and benzol.

10. *Experiments with Wimshurst Machine.*—It has already been stated that the curves obtained by the aid of the machine are much inferior in regularity to those given by the induction-coil; in fact, without care they cannot be produced at all. But there are some other peculiarities which deserve notice. Thus, while with every coil used and with varying battery-power it was found that magnesia alba gave 80 lines to the inch, when the machine was used the number varied in different parts of the figure from 56 to 64. There was no particular mode of variation, that is, no crowding together at a special place, &c. This irregularity in the numbers is not, however, a general thing; for in other cases tried, the numbers did not vary among themselves and agreed with those obtained with the coils.

The common centre of the circles, instead of being, as with the coil, halfway between the terminals, seemed to be a little nearer the positive electrode than the negative. Other experiments are, however, necessary before this can be regarded as established.

In experimenting with this machine it is necessary to avoid placing the plate too near the terminals, otherwise the powder becomes electrified, and the plate also, and the resulting figures are combinations of those showing the distribution of the electricity upon the plate, and the undulatory effect discussed in this paper.

Most of the figures obtained with the machine were caused by the spark passing between knobs. They are imperfect at the sides, in consequence of the undulatory effect being prevented from making itself felt in those directions by the solid body of the knob coming between. Thus the portion of the circles shown are at right angles to the direction of the spark. The figure can of course be obtained from sparks taken between points, but it is more difficult. It was particularly noticed in the course of these and some other experiments that, whilst substituting a knob for a point in a Wimshurst or other machine increased the length of spark obtainable, it was just the opposite with a coil; for here the spark is considerably reduced if taken between *two* knobs. The longest spark with a coil is obtained either between two points, or between one point and one knob or plate.

It was also noticed that under the positive electrode the powder was scattered so as to cause a space. This is sometimes, but not always, the case under the negative.

11. *Production of the Cleared Space.*—It will be observed that the more or less elliptical space in the middle of the figures is not present in all. It is only produced when the plate is held close up to the terminals and when the vigour of the spark is considerable. At a first glance at a figure containing the clear space, it appears that the lines are crowded together at the ends of the major axis of the ellipse. Closer examination and careful measurement will show, however, that this is not the case, for the number of lines per unit of length is the same whether measured at the ends of the major or of the minor axis. In fact the appearance is just that of concentric circles, formed of the powder, with the material

composing the smaller circles swept up so as to form the irregular ellipse.

From this fact, and also that the circles can be produced without any clearing of the powder, I am led to conclude that the production of the circles is due to *an entirely different cause* to that producing the space, but that both are necessary accompaniments of the disruptive discharge.

12. *Discussion of Results.*—When these figures were first obtained it was imagined that they were due to the sound which accompanied the spark, and in fact gave a graphical representation of the sound-waves. This view was to some extent confirmed by finding that the same substance gave the same number of waves in the same space. If this be so, however, then there ought to be no difference in the numbers obtained with different powders; for the sound of the spark produced by a coil is remarkably constant, and, if such a term can be rightly applied to it, it is of unvarying pitch. This view, however, is also disproved by the measurements; for if it be taken that the lines show the phases of the undulatory movement, then the longest distance between similar phases is that given by powdered chalk, viz. $\frac{1}{40}$ of an inch. But the upper limit of vibration producing a sound-wave may be taken as equal to 40,000 per second, giving a length for the wave of as nearly as possible one third of an inch, a distance thirteen times greater than the longest given by these experiments. It is therefore evident that the vibrations producing these curves and those producing the sound of the spark are not identical.

From the fact that the number given by a mixture is intermediate between those of its constituents, we should be led to conclude, since the density of a mixture follows the same law, that therefore this is the factor which regulates the number given by any particular substance. If this be the case, then the powders should arrange themselves in a series, with those of the highest density at the top and those of the lowest at the bottom. A glance at the tables of results will, however, show that no such order is observed. Thus we have silica giving a higher number (88) than the heavy oxides of lead (64); magnesia alba, a substance of very low specific gravity, appearing high up in the list; and tannin, also of low specific gravity, on the other hand, appearing low down. What conditions, if any, regulate the number of these lines I have hitherto been unable to determine. Before any generalization can be made it is necessary to examine a very much larger number of substances, and experiments are being conducted with this purpose. At present, indeed, I am not disposed to attach much importance to the absolute length of any one of these undulations, but rather to the great similarity of the measurements as a whole. For although it is likely that errors may occur in some of the individual members, it is hardly possible that the whole are erroneous. It will therefore follow that undulations of a length of about one sixty-fourth of an inch (taking the mean measurement) accompany the disruptive discharge.

The photographs from which the figures in the Plate are copied were taken for me by Mr. Dunscombe, of Bristol, and were printed from the negatives without the latter being touched in any way.

Note, June 9, 1888.—Since the above was written, I have been informed by Prof. Rücker that he has an apparatus in his possession, made by the late Dr. Guthrie, which gives somewhat similar results. This was copied from one exhibited at the Loan Exhibition of Scientific Apparatus in 1879. It consists of an elliptical dish with vertical walls. Sand is scattered over the bottom, a glass plate placed over the top, and a series of electric sparks produced at one focus of the ellipse. The sand will then be found to arrange itself in a series of circular curves around the other focus. The curves thus produced bear a striking resemblance to those exhibited in the Plate accompanying this paper, and are of course due to the same cause.

XLIX. *On a new Form of Lantern.*

By W. Lant Carpenter, *B.Sc.*

Mr. W. Lant Carpenter exhibited (June 9, 1888) a new form of Lantern, recently constructed by Mr. Hughes, of Dalston. The mahogany body is hexagonal, and each of the three front sides is provided with condensers and projecting arrangements. The back opens to give access to the radiant, which in this case is a Brockie-Pell Arc-Lamp; but, if necessary, a lime-light can be readily substituted. The lamp is fixed to the base-board, and the body can be rotated through 60° on either side of the central position, thus allowing any of the three nozzles to be directed towards the screen. The three sets of condensers are placed so that their axes intersect at a point about which the radiant is placed. The centre nozzle is fitted as a lantern-microscope, with alum cell and various sets of condensing-lens and objectives, and a space in front of the main condensers is provided for polarizing-apparatus. The focusing arrangement consists of a skew rack and pinion and a fine screw-adjustment; and the whole microscope can be easily removed and a table-polariscope substituted. The right-hand nozzle is arranged for the projection of ordinary lantern-slides, and the left-hand one is provided with an adjustable slit for spectrum work. A small table sliding on rails serves to carry the prisms, and the same rails support projecting lenses.

Prof. S. P. Thompson congratulated Mr. Lant Carpenter on his selection of the Brockie-Pell lamp as the radiant; for, in addition to its being a focusing-lamp, it is unique in the fact that it works satisfactorily on either constant-current or constant-potential circuits.

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7

PROCEEDINGS

AT THE

MEETINGS OF THE PHYSICAL SOCIETY

OF LONDON.

SESSION 1887-88.

February 12th, 1887.

Prof. Balfour Stewart, President, in the Chair.

The following was elected a Member of the Society:—

Mr. John Brown.

The following communication was made:—

"On the Tenacity of Spun Glass." By Messrs. E. Gibson and R. Gregory.

February 26th, 1887.

Prof. W. G. Adams, Vice-President, in the Chair.

The following were elected Members of the Society:—

Mr. C. S. Galbenkian; Prof. W. Stroud.

The following communications were made:—

"Note on Prof. Carey Foster's Method of Determining the Coefficient of Mutual Induction of two Coils." By Mr. James Swinburne.

"On the Determination of Coefficients of Mutual Induction by an Earth Inductor and a Ballistic Galvanometer." By Mr. R. H. M. Bosanquet.

"On Evaporation and Dissociation.—Part VI. The Continuous Transition from the Liquid to the Gaseous State of Matter at all Temperatures." By Prof. W. Ramsay and Dr. Sydney Young.

March 12th, 1887.

Prof. G. Carey Foster, Vice-President, in the Chair.

The following communications were made:—

"On the Electrical Conductivity of Vertically Suspended Wires." By Mr. Shelford Bidwell.

"A Lecture Experiment on Self-Induction." By Mr. Shelford Bidwell.

"A Lecture Experiment for proving the Law connecting the Capacity of an Electric Condenser with the Distance between its Coatings." By Profs. Ayrton and Perry.

"Note on Magnetic Resistance." By Profs. Ayrton and Perry.

March 26th, 1887.

Prof. Balfour Stewart, President, and afterwards Prof.
W. G. Adams, Vice-President, in the Chair.

The following was elected a Member of the Society:—

M. Edmond van Aubel.

The following communication was made:—

"On the Production and Properties, and on some suggested Uses of the Finest Fibres." By Mr. C. Vernon Boys.

April 23rd, 1887.

Prof. W. G. Adams, Vice-President, in the Chair.

The following communications were made:—

"On Delicate Calorimetrical Thermometers; and on the Expansion of Thermometer Bulbs under Pressure." By Prof. S. U. Pickering.

"Note on Magnetization." By Mr. R. H. M. Bosanquet.

"On a Thermodynamical Relation." By Prof. W. Ramsay and Dr. Sydney Young.

May 14th, 1887.

Prof. W. E. Ayrton, Vice-President, in the Chair.

The following was elected a Member of the Society:—

Mr. T. Mather.

The following communications were made:—

"On a Modification of a Method of Maxwell's for Measuring the Coefficient of Self-Induction." By Mr. E. C. Rimington.

"On the Production of Sudden Changes of Torsion in a Wire by Changes of Temperature." By Mr. R. H. M. Bosanquet.

"On a Magnetic Potentiometer." By Mr. A. P. Chattock.

May 28th, 1887.

Prof. W. E. Ayrton, Vice-President, in the Chair.

The following communications were made:—

"On Transformers for Electrical Distribution." By Prof. S. P. Thompson.

Prof. Ayrton exhibited an experiment illustrating the principle of Transformers.

"On the Magnetic Torsion of Iron Wires." By Mr. Shelford Bidwell.

June 11th, 1887.

Mr. Shelford Bidwell, Vice-President, in the Chair.

The following communications were made:—

Exhibition of Puluj Vacuum Tubes." By Mr. Warren De La Rue.

"Note on Beams fixed at both Ends." By Profs. Ayrton and Perry.

"Note on MM. Vaschy and De la Touanne's Method of Determining Coefficients of Mutual Induction." By Prof. Carey Foster.

"Note on the Relation between the Angle through which a Twisted Steel Strip is Uncoiled, and the Load." By Prof. J. Perry.

June 25th, 1887.

Mr. Shelford Bidwell, Vice-President, in the Chair.

The following communications were made :—

"Note on Magnetic Resistance." By Profs. Ayrton and Perry.

"On Sounding Coils." By Prof. W. Stroud and Mr. J. Wertheimer.

"On the Comparison of Electrical Capacities." By Mr. E. C. Rimington.

"On the Effects of Change of Temperature in Twisting or Untwisting Wires which have suffered Permanent Torsion." By Mr. H. Tomlinson.

"On Permanent-magnet Ammeters and Voltmeters with Invariable Sensibility." By Profs. Ayrton and Perry.

November 12th, 1887.

Prof. W. E. Ayrton, Vice-President, in the Chair.

The following was elected a Member of the Society :—

Lieutenant R. H. S. Bacon, R.N.

The following communications were made :—

"On a Geometrical Method of Determining the Conditions of Maximum Efficiency in the Transmission of Power by Alternating Currents." By Mr. T. H. Blakesley.

"A Lecture Experiment on the Velocity of Sound." By Prof. A. W. Rücker.

November 26th, 1887.

Prof. Balfour Stewart, President, in the Chair.

The following was elected a Member of the Society :—

Asutosh Mukhopadhyay, M.A., F.R.A.S.

The following communications were made:—

"On the Analogies of Influence- and Dynamo-Machines." By Prof. S. P. Thompson.

"On the Effect produced on the Thermoelectric Properties of Iron when under Stress or Strain by Raising the Temperature to Bright Red." By Mr. H. Tomlinson.

"On a Method of Discriminating between Real and Apparent Coincidences of the Lines of Different Spectra; with some Applications." By Mr. E. F. J. Love.

December 10th, 1887.

Prof. W. E. Ayrton, Vice-President, in the Chair.

The following were elected Members of the Society:—

Mr. C. A. Carus-Wilson; Mr. W. E. Sumpner, B.Sc.

The following communications were made:—

"On the Optical Properties of Phenyl-thio-carbimide." By Mr. H. G. Madan.

"On the Recalescence of Iron." By Mr. H. Tomlinson.

"On the Rotation of a Copper Sphere and of Copper-wire Helices when freely suspended in a Magnetic Field." By Dr. Shettle.

January 28th, 1888.

Prof. W. G. Adams, Vice-President, in the Chair.

The Chairman reported the death of the President, Prof. Balfour Stewart, which took place on the 18th December, 1887.

The following communications were read:—

"On the Effect of Magnetization on the Thermoelectric Properties of Bismuth." By Mr. H. M'Leod.

"On the Influence of Magnetism and Temperature on the Electric Properties of Bismuth, and of its Alloys with Tin and Lead." By M. E. van Aubel.

"On a Water-Dropping Influence-Machine." By Prof. S. P. Thompson.

"On the Price of the Factor of Safety in Lightning Rods." By Prof. S. P. **Thompson**.

"On the Optical Demonstration of Electrical Stress." By Prof. A. W. **Rücker** and Mr. C. V. **Boys**.

Annual General Meeting.

February 11th, 1888.

Dr. **Gladstone**, F.R.S., Vice-President, in the Chair.

The following Report of the Council was read by the Chairman :—

At the Annual General Meeting last year the Council, in presenting their Report to the Society, had to record the death of Dr. Guthrie, who had taken a leading part in the formation and work of the Society, and who died only eight months after vacating the Presidential Chair. This year the Council regret to have to report the death of Dr. **Balfour Stewart**, who would to-day, had he survived, have completed his two years' term of office as President. Dr. Stewart was well known to most of us ; and, though living at a considerable distance from London, was not unfrequently present at and presided over our Meetings. He died very suddenly on the 18th of December last, at Ballymagarvey, near Dublin, whither he had gone to spend Christmas with his family.

The Council have also to record the deaths of Professor Kirchhoff, of Berlin, who was elected an Honorary Member of the Society in 1879 ; of Mr. Coutts Trotter, Fellow of Trinity College, Cambridge ; and of Prof. Humpidge, of University College, Aberystwith.

Since our last Annual Meeting a Bust of the late Dr. Guthrie, kindly presented to the Society by Mrs. Guthrie, has been placed on a bracket in the room in which our Meetings are held.

The Council consider that the work of the Society during the past year has been satisfactory. The Meetings have been well attended, and have often been prolonged beyond the limits usually assigned to the scientific business of a Meeting. The papers brought before us have not fallen short, either in number or general interest, of those of previous years, and have frequently given rise to useful discussions. As many of these communications, however, are of the nature of notes or lecture experiments, or refer to investigations published elsewhere, the amount of printed matter in the quarterly parts of our 'Proceedings' is somewhat less than was the case last year. The Council would again impress upon Members the importance of bringing their original researches in the first instance before this Society. The advantages they thereby gain are early and wide publication of their papers, and an ample supply of extra copies furnished at the expense of the Society.

The Council regret to announce that the number of Members has not increased during the last year. Our losses by death and withdrawal have only just been compensated by the new Members elected. There are, doubtless, many students of Physics in this country, and others interested in the objects of our Society, who would be willing to join us, if they were aware of the advantages which we offer them.

The Council have determined to issue from time to time translations of important Physical Memoirs published abroad. In deciding to enter upon this undertaking the Council have been influenced by the consideration that there is not at present any channel through which the longer and more elaborate publications of Continental Physicists become accessible to a large number of their English fellow-workers. The extent of the Society's action in this direction will chiefly depend upon the amount of funds available for the purpose, for the Society can hardly hope to do more than make a selection among the many valuable foreign memoirs which it would be desirable to republish in this country.

These translations will be printed and got up uniformly with the 'Proceedings of the Physical Society,' but will be paged separately. The first Part, of over 100 pages, containing a series of five papers by Professor von Helmholtz, on the Application of the Principles of Thermodynamics to Chemical Action and Electrolysis, is in an advanced state of preparation, and will be issued to Members very shortly. It will be followed as soon as practicable by a translation of Hittorff's two memoirs on the Discharge of Electricity through Rarefied Gases; and by a translation of Puluj's memoir on Radiant Electrode-Matter, which has been kindly promised to the Society by Dr. De La Rue.

Negotiations are now in progress for the final revision and production of the translation of Volta's works which the Society has undertaken.

Since the last Annual Meeting the Bye-Laws have been amended in such a way as to allow us to elect, as Members of the Society, gentlemen residing abroad, who are personally unknown to our Members, but otherwise qualified for election. Under the new rule two new Members have been elected—one residing in Belgium, the other in India.

In order to facilitate the investment of the Society's funds, the Council in June last resolved to procure a Seal for the Society, under which future investments might be made. A small Committee was appointed to consider the matter, and a simple design was selected and approved by the Council. The Seal will remain in the custody of the Treasurer and one other Member of Council, and future investments will be made in their name under the Corporate Seal.

The Library now contains about 1000 volumes, consisting largely of Periodicals and the Proceedings of Learned Societies. The Council wish it to be known that the Library is open to all Members of the Society for consultation of books, and that Members may take away bound volumes.

The following additions to the Library have been made during the past year:—

Journal of the Society of Arts.
Quarterly Journal of the Royal Meteorological Society.
Proceedings of the Academy of Natural Sciences, Philadelphia.
Journal of the Society of Telegraph Engineers.
Proceedings of the Institute of Mechanical Engineers.
Journal of the College of Science, Japan.
Journal of the Physical and Chemical Society of Russia.
Johns Hopkins University Circular.
Séances de la Société Française de Physique.
Proceedings of the Cambridge Philosophical Society.
Proceedings of the Royal Institution.
Journal and Proceedings of the Royal Society, N. S. Wales.
Transactions of the Seismological Society, Japan.
Crónica Cientifica.
Electrical Review.
Beiblätter der Physik und Chemie.
Annalen der Physik und Chemie.
Journal de Physique.
Scientific News.
The Naturalists' Monthly.
Electrical Plant.
Ingeniero y Ferretero Español.
Indian, Colonial, and Foreign Engineer and Builder.

Kew Observatory Report, 1887.
Catalogue of the Library of the Institution of Mechanical Engineers.
List of Members of the Royal Institution.

The Council have approved the following

RULES FOR THE REGULATION OF THE LIBRARY.

1. Books taken from the Library must be returned within three months, when they may be again borrowed unless wanted by another Member.
2. If a book in possession of one Member is wanted by another, it must be returned within a week; but if it has not been borrowed for more than a fortnight, it may be retained for a period of three weeks from the time of borrowing.
3. Unbound books or parts of periodicals may be consulted in the Library, but may not be taken away.
4. Books cannot be sent out; they must, in all cases, be taken away by a Member, or by some person with his written authority, after which such Member will be responsible for their condition and safety.
5. In no case may books be taken away unless the particulars are entered in a book kept for the purpose.
6. The total number of books that a Member may have out at any one time is limited to four.

Fines.

1. A fine of One Shilling per volume shall be imposed for every week, or fraction of a week, that a book is detained beyond the time fixed for its return (notice being sent); and the use of the Library shall be withdrawn till the fine is paid in full. Money received as fines shall be used for the benefit of the Library.
2. If a borrowed book be lost or damaged, the borrower shall replace it, or pay the full value of the set to which it belongs (such value to be estimated by the Council) ,or any sum less than the full value, at the discretion of the Council.

NOTICES OF DECEASED MEMBERS.

Dr. BALFOUR STEWART was born in Edinburgh on November 1, 1828. His uncle, Dr. Cloaston, the minister of Stanwick, was a well-known naturalist and meteorologist. Having studied for a short time at each of the Universities of St. Andrews and Edinburgh, Stewart began practical life in a mercantile office. In the course of a business voyage to Australia, his particular taste for physical science developed itself; and his first published papers, "On the Adaptation of the Eye to Different Rays," and "On the Influence of Gravity on the Physical Condition of the Moon's Surface," appeared in the 'Transactions of the Physical Society of Victoria.' On his return he gave up business for science, and resumed study under Kelland and Forbes, to the latter of whom he soon became assistant. During this period, in 1858, Stewart was led to his well-known extension of Prévost's 'Law of Exchanges,' a most remarkable and important contribution to the theory of radiation. He seems to have been the first to suggest, from a scientific standpoint, that radiation is not a mere surface phenomenon. With the aid of Forbes's apparatus, he fully demonstrated the truth of the conclusions to which he had been led by theory; and the award of the Rumford Medal by the Royal Society, some years later, showed that his work had been estimated at its true value. In fact, his proof of the necessary equality between the radiating and the absorbing powers of every substance remains to this day the simplest, and therefore the most convincing, that has yet been given.

On the death of Mr. Welsh, in 1859, Balfour Stewart was selected to succeed him as Superintendent of the Kew Observatory, and held the office until 1871. During these years he maintained and improved upon the memorable labours of Ronalds and Welsh. Every species of inquiry which had to be carried out at Kew—whether it consisted in the testing of thermometers, sextants, pendulums, aneroids, or dipping-needles, the recording of atmospheric electricity, the determination of the freezing-point of mercury or the melting-point of paraffin, or the careful study of the peculiarities of the air-thermometer—received the benefit of his valuable suggestions, and was carried out with his scrupulous accuracy.

In 1867 Stewart was appointed Secretary of the Government Meteorological Committee; and in the following year superintended the erection of the instruments at the several stations of the Meteorological Office—Aberdeen, Armagh, Falmouth, Glasgow, Stonyhurst, and Valencia. At the close of the year 1869 he resigned the Secretaryship of the Meteorological Committee.

In 1870, on his appointment to the Chair of Natural Philosophy in Owens College, Dr. Stewart left Richmond, to take up his residence in the neighbourhood of Manchester; but continued to act as

Superintendent of the Kew Observatory. It was in November of this year that, in one of his journeys between London and Manchester, he met with the railway accident which so nearly proved fatal to him. His thigh was dreadfully crushed; and it was nine months before he could be removed from Harrow, where the disaster occurred. He was permanently lamed, and sustained severe injury to his constitution. From the vigorous activity of the prime of life, he passed in a few months to grey-headed old age. But his characteristic patience was unruffled and his intellect unimpaired.

He was much interested in the phenomena of sun-spots, and their connexion with the earth's magnetism. In 1866 the first set of results obtained from the photo-heliograph was published, at the expense of Dr. Warren De La Rue, under the title of "Researches on Solar Physics," by Messrs. De La Rue, B. Stewart, and B. Loewy. This was followed by papers on Sun-spot areas &c., published in the same manner in 1867 and 1868. The results of these researches were also communicated to the Royal Society.

Of recent years Dr. Stewart's attention had been turned to the difficult problem of terrestrial magnetism and its variations. One of his last papers read before this Society investigated the causes of the solar diurnal variation of the earth's magnetism. In his latest remarks addressed to us, on November 26th, he made reference to this subject, and hinted at the probability that before long many of its obscure phenomena would receive an explanation. The article on "Terrestrial Magnetism" in the new edition of the 'Encyclopædia Britannica' was contributed by him.

Dr. Stewart was elected a Fellow of the Royal Society in 1862, and was awarded the Rumford Medal in 1868. He was President of Section A of the British Association in 1875, at the Bristol Meeting. He was an original Member of our Society.

For two periods he acted as Examiner in Natural Philosophy in the University of London, and he also examined for the University of Edinburgh and other Examining Boards.

When in Richmond, Dr. Stewart was Churchwarden of St. John's Church, School Manager, Superintendent of the Boys' Sunday School, and generally an active coadjutor of the Vicar (Canon J. D. Hales) in parish matters. He was interested, as is well known, in psychical science, and was President of the Society for Psychical Research. The philosophic and speculative tone of his mind was well illustrated in the 'Unseen Universe,' which he wrote in conjunction with Prof. P. G. Tait. This work, which appeared in

1874, excited a profound interest, and passed through many editions. In it the authors combated the view that science and religion are incompatible with each other, and contended, from a purely physical point of view, for the possibility of immortality and a personal God.

As a writer of text-books on Physics, Dr. Balfour Stewart was very successful. Among these may be mentioned his 'Elementary Treatise on Heat,' published in 1866; 'Lessons in Elementary Physics' (1871); 'Physics' (1872); and 'The Conservation of Energy' (1874). Of his work on 'Practical Physics,' written in conjunction with Mr. Haldane Gee, two volumes have already appeared, and the third volume is in an advanced state of preparation.

At Manchester Prof. Balfour Stewart saw the establishment of the Victoria University, and took an active part in the arrangement of the academical curriculum.

He died very suddenly at Ballymagarvey, near Navan, co. Meath, Ireland, on the 18th of last December.

GUSTAV ROBERT KIRCHHOFF, Honorary Member of the Society, was born in Königsberg, in March 1824, the son of a lawyer of that town. At eighteen he entered the University of Königsberg, where he attended the mathematical lectures of Richelot, whose daughter he afterwards (1857) married; and studied Physics under Franz Neumann, who, directly or indirectly, has been the teacher and inspirer of almost every mathematical physicist of Germany. At the age of twenty-one, while still a pupil of Neumann, Kirchhoff published his celebrated investigation into the flow of electricity in a plane conducting sheet. At twenty-three he took his Doctor's Degree with so much distinction that, as a mark of approval, a fund was awarded to him to defray the cost of a scientific journey to Paris. This was in 1848. Being prevented by the political unrest of the time from carrying out his intended journey, Kirchhoff connected himself with the University of Berlin as *Privatdocent*. It is noteworthy that in the University, where afterwards his lectures were attended by hundreds of eagerly attentive listeners, the first course announced by him was not delivered for want of an audience. In 1850 Kirchhoff was called to Breslau as Extraordinary Professor of Physics. Here began an afterwards unbroken friendship with Bunsen, which influenced in important respects the outward course of Kirchhoff's subsequent life as well as the direction of his scientific activity. On Gmelin's death, Bunsen went to Heidelberg as Professor of Chemistry; and when, in 1854, Jolly left Heidelberg for Munich, the Ordinary Professorship of Physics in Heidelberg was, by Bunsen's advice, offered to Kirchhoff. This post was retained by Kirchhoff for twenty-one years; until, in 1875, he at last accepted a call, the third that had been addressed to him, to the University of Berlin. It was in Heidelberg, in constant fellowship with his friend Bunsen, and partly in definite conjunction with him, that Kirchhoff's researches into the nature of the solar spectrum and its relation to the spectra of the light from artificial sources were carried out. These investigations, which have attracted far more general attention than any other part of Kirchhoff's work, supplied the key whereby, in the hands of Kirchhoff himself, followed by Huggins, Lockyer, Janssen, and others, stores of knowledge have been unlocked concerning the sun and other heavenly bodies, which previously had been supposed to be for ever inaccessible. The starting point of these discoveries was the establishment of the relation between the dark lines of the solar spectrum and the bright lines of the spectra of artificial sources of light. Kirchhoff having, with Bunsen, determined the equal refrangibility of the dark solar line D and the bright yellow line given by a sodium-flame, was observing the D-line, and introduced a flame coloured with salt into the field of view of the spectroscope at the same time as light from the sun, expecting now to see a bright line in the place of the dark line previously visible. And this indeed happened with diffused light from the clouds; but when the experiment was made with direct, full sunshine, the dark line, instead of being changed to bright, became darker and broader than before. He left the laboratory saying, "That seems to me to go to the root of the matter" [" *das scheint mir eine fundamentale Geschichte* "]. The next day he had found the explanation, and had arrived at his general theorem of the relation between emission and absorption.

The whole of Kirchhoff's experimental work is of the highest order, and is marked always by originality and precision; but it was as one of the small band of leaders in mathematical physics that his extraordinary powers were chiefly exerted. His University lectures, which were of the kind that constitute the glory of the German University system, represent an important part of his contribution to the progress of science. "His lectures were quiet, clear, carefully thought out, with not a word too much, and not one too little." They were models of absolute precision of statement and logical consistency. He strove to get rid of whatever was indefinite, of every merely subjective judgment, of every customary phrase used without clearly defined meaning. In relation to such a subject, these statements characterize the matter and quality of thought much rather than mere style of composition. The progress of knowledge in mathematical physics, even more than in other branches of science, is essentially concerned with the recognition of how much in accepted statements of the results of observation is merely conventional and unnecessary. Here, more then elsewhere, precision of statement implies and generates precision of thought, and to rid the language of science of unmeaning or needless phrases is to advance knowledge.

For the last twenty years or so of his life Kirchhoff's health was much impaired, and his bodily activity interfered with by the results of a fall. He was seldom quite well afterwards, and for a long time he was able to get about only on crutches or in a wheelchair.

His first wife died in 1869, leaving him two sons and two daughters. At Christmas, 1872, he married again. His second wife, who survives him, was Miss Louise Brömmel, at the time of her marriage Matron of the Ophthalmic Hospital at Heidelberg.

His death occurred peacefully, but not without warning, on the 17th October, 1887.

[The above notice is compiled from the eloquent oration delivered in memory of Kirchhoff by Professor Ludwig Boltzmann, of Graz, in November 1887; and from an article in the 'Deutsche Rundschau' for February 1888, by R. von Helmholtz.]

Coutts Trotter came up to Trinity College, Cambridge, from Harrow, at the age of eighteen, in the year 1855. He was elected a Scholar of the College at the earliest opportunity, and took his Degree in 1859, with honours both in Classics and Mathematics. He shortly afterwards became a Fellow of his College, and proceeded to Heidelberg to continue the study of Natural Science, which had interested him since the time when he listened to the Royal Institution Lectures of Faraday. While there he worked for some time at Physiology, under Helmholtz, and acquired considerable acquaintance with Botany and Chemistry. Soon after his return, he became Natural Sciences Lecturer at Trinity; and after that, though he delivered some lectures on Botany, he confined his attention mainly to Physics. He became a Member of our Society in 1879. The position of the Natural Sciences in Cambridge has changed immensely in the last twenty-five years. The Honour Examinations, admitting to the degree of B.A., were first held in 1861, when three names appeared in the Class List; and in framing the lines along which the progress should take place, no man had so prominent a share as Trotter.

His wide acquaintance with literature, as well as with science, his great power of handling and arranging business details, his patient care and perseverance, his readiness to listen to his opponent's case, with his evident desire to reconcile in a fair and honest way conflicting claims, gave him an influence in the University which it will be difficult to replace; and by his death Scientific Research and Education in Cambridge has lost one of its ablest advocates. He did not contribute many papers to Scientific Journals or Societies. A few years since he communicated one to the Cambridge Philosophical Society (of which he was President at the time of his death), on Phenomena connected with the Melting of Ice. Another paper, containing the Results of Experiments on Ice at its Melting-point, carried out in the Ice Grotto at Gründelwald, was read before the Royal Society in 1886. Among his large and valuable collection of apparatus, which ~~he~~ left after his death to Newnham College, were some instruments ready packed for transit abroad in order to continue this research; but his illness prevented him from fulfilling his intention.

His name will live not through his scientific work, but in the statutes and regulations which govern University life, and which bear the impress of his wise judgment.

He had been seriously unwell during last winter and early spring; but in the summer he seemed almost to have regained his strength. When he returned from abroad in October, however, it was clear to his friends that there was too good cause for alarm; and an attack of inflammation of the lungs brought about the end on December 4th.

Thomas Samuel Humpidge was born at Gloucester, July 23rd, 1853. He was educated at the Crypt Grammar School, and in his fifteenth year entered a corn-merchant's office. Finding, however, that his tastes lay in the direction of scientific rather than commercial pursuits, he industriously gave all his leisure hours to study, and attended the local evening classes in connexion with the Science and Art Department. In 1873 he won the Silver Medal in Geology, and was awarded a Royal Scholarship at the Royal School of Mines.

He entered that Institution in 1874, and devoted himself chiefly to the study of Chemistry, under Professor Frankland. The year after he gained one of the Jodrell Scholarships, which enabled him in 1876 to proceed to the University of Heidelberg, where he studied under Professor Bunsen. In 1877 he passed the B.Sc. degree of the University of London, and in 1878 took his degree of Ph.D. (*summâ cum laude*). The same year he undertook teaching duties at the Fellenberg Institute at Hofwyl, near Berne, a post which he retained until appointed Professor of Natural Science in the University College of Wales, at Aberystwyth, in September 1879. Under the very cramped financial conditions of the Aberystwyth College at that date, the duties of Dr. Humpidge's Chair were truly exacting. Single-handed, he was expected to lecture on Chemistry, Physics, and Biology. After two years he was relieved of the last subject, and in 1884 of Physics. Nevertheless, whilst thus overweighted with teaching-work, he found time not only to organize on very slender means a fair Chemical Laboratory, but also to engage in research. All this proved a severe tax on his health and strength; and when, after the destruction of the College by fire in August 1885 (from which he narrowly escaped with his life), he redoubled his energies to organize the temporary laboratories, his weakened constitution sustained a shock from which it never recovered. While at Heidelberg he had specially studied the properties of the rare metals Yttrium and Erbium. At Aberystwyth he had devoted himself to a study of Beryllium, the atomic weight of which was at that time in doubt. This he redetermined: first, from its specific heat, and, secondly, from the vapour-density of its chloride and bromide. Finding the second method to produce results differing from those of the first method, he patiently repeated the calorimetric experiments, and had the satisfaction of proving that the specific heat of beryllium, like that of carbon, of boron, and of silicium, is abnormal at low temperatures, and only agrees with the law of Dulong and Petit as the temperature is raised. In the course of this research he constructed some new and elaborate calorimetric apparatus, with which he proposed to redetermine the specific heats not only of the rare elements, but of copper, tin, and other metals of which he had prepared specimens in a state of great purity. Unfortunately, not only these specimens, but the whole of his apparatus, as well as the manuscript records of many months of research, were destroyed in the College fire in 1885. Undaunted, he began again, reconstructed his apparatus, and prepared fresh pure specimens for the calorimetric work. His health, however, broke down early in 1886; and he spent the autumn and the winter of 1886–87 in the south of Europe. At Easter, 1887, he returned to England, little fit for work, but apparently on the high road to recovery. Suddenly, in the autumn, cerebral deterioration set in, and he expired on November 30th, 1887. In addition to sundry memoirs and papers published in the 'Proceedings' and in the 'Philosophical Transactions' of the Royal Society, and in the 'Journal' of the Chemical Society, he published in 1884 a translation of Kolbe's 'Inorganic Chemistry.' He leaves a wife and two young children to mourn his loss.

The Society then proceeded to the election of Officers and Members of Council; and the new Council was constituted as follows:—

President.—Prof. A. W. REINOLD, F.R.S.

Vice-Presidents who have filled the office of President.—Dr. J. H. GLADSTONE, F.R.S.; Prof. G. C. FOSTER, F.R.S.; Prof. W. G. ADAMS, M.A., F.R.S.; Sir WILLIAM THOMSON, D.C.L., LL.D., F.R.S.; Prof. R. B. CLIFTON, F.R.S.

Vice-Presidents.—Dr. E. ATKINSON; Prof. W. E. AYRTON, F.R.S.; SHELFORD BIDWELL, M.A., LL.B., F.R.S.; Prof. H. MCLEOD, F.R.S.

Secretaries.—WALTER BAILY, M.A.; Prof. JOHN PERRY, F.R.S.

Treasurer.—Prof. A. W. RÜCKER, F.R.S.

Demonstrator and Librarian.—C. VERNON BOYS.

Other Members of Council.—Hon. R. ABERCROMBIE; R. H. M. BOSANQUET, M.A.; W. H. COFFIN; CONRAD W. COOKE; Prof. F. FULLER; W. N. SHAW, M.A.; A. STROH; Prof. S. P. THOMPSON, D.Sc.; H. TOMLINSON; G. M. WHIPPLE.

Votes of thanks were passed to the Lords Committee of the Council on Education; to the OFFICERS; and to the AUDITORS.

THE TREASURER IN ACCOUNT WITH THE PHYSICAL SOCIETY, FROM JANUARY 1st, 1887, TO DECEMBER 31st, 1887.

Dr.	£	s.	d.	£	s.	d.
Balance in Bank, January 1, 1887				352	14	10
Entrance-Fees	14	0	0			
Subscription for 1881	1	0	0			
Subscriptions for 1882	2	0	0			
" 1883	5	0	0			
" 1884	6	0	0			
" 1885	16	0	0			
" 1886	39	0	0			
" 1887	135	0	0			
Excess Subscriptions	0	17	0			
Five Life Subscriptions	50	0	0			
				268	17	0
One year's Dividend on £400 Furness 4 per cent. Debenture Stock, less Income Tax	15	9	8			
One year's Dividend on £575 Midland 4 per cent. Preference Stock, less Income Tax	22	5	2			
One year's Dividend on £200 Lancaster Corporation Stock, less Income Tax	7	14	10			
One year's Dividend on £200 Metropolitan Board of Works Stock, less Income Tax	6	15	8			
				52	5	4
Received from Taylor and Francis :—						
Sales in 1886	14	0	0			
Sales in 1887	51	11	0			
				65	11	0
Balance due to Treasurer				3	12	3
				£743	0	5

Cr.	£	s.	d.	£	s.	d.
Balance due to Treasurer				3	7	8
Excess Subscriptions returned				0	17	0
Bank charges				0	3	9
Taylor and Francis :—						
Proceedings, vol. viii. (parts 1–4)	94	19	6			
Circulars, postage and addressing	20	2	0			
Members' separate copies	20	13	6			
Miscellaneous printing	22	10	9			
Joule, vol. i.	2	10	0			
" vol. ii.	140	8	10			
				301	4	7
Gyde, for Binding				17	16	6
Haggard, for Binding				7	0	9
Index to Joule				15	0	0
Good, for Corporate Seal				2	11	0
Attendance at Meetings and incidental expenses				7	7	6
Invested in £254⅞ New South Wales 3½ per cent. Stock, with expenses				250	0	0
Petty Cash :—						
Dr. Atkinson				2	13	6
Prof. Reinold				1	14	8
Prof. Rücker				4	4	3
Balance in Bank				128	19	3
				£743	0	5

Audited and found correct,

GEORGE FULLER,
RICHARD INWARDS, } *Auditors.*

London, January 20th, 1888.

PROPERTY ACCOUNT OF THE PHYSICAL SOCIETY, December 31, 1887.

Assets.	£	s.	d.
Balance in Bank	128	19	3
Subscriptions due	88	0	0
£400 Furness 4 per cent. Debenture Stock	456	0	0
£575 Midland 4 per cent. Preference Stock	650	0	0
£200 Metropolitan Board of Works Stock	217	0	0
£200 Lancaster Corporation Stock	224	0	0
£254⅜ New South Wales 3½ per cent. Stock	249	7	5
	£2013	6	8

Liabilities.	£	s.	d.
Balance due to Treasurer	3	12	3
Bills outstanding:—			
Taylor and Francis (one Part of Proceedings)	57	18	10
Williams and Norgate	4	16	0
Balance	1946	19	7
	£2013	6	8

We have examined the above Account, and also the Securities at the Bank, and find the same to be correct.

GEORGE FULLER,
RICHARD INWARDS, } *Auditors.*

London, January 20th, 1888.